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I. THE DAWN OF LIGHT PRINTING.

IF the question were asked—Who discovered photography?—in very few instances, do we think, would the reply be a correct one. It would not, of course, be just, as we all know very well, to attribute to any one individual the invention of the art as practised at the present moment, for many minds and many hands had to be engaged on the subject before the methods we are to-day so well acquainted with were elaborated; and obviously, therefore, to no one of the illustrious names connected with the early history of photography can the undivided honour of being sole discoverer be assigned. Wedgwood and Davy were undoubtedly among the very first to show that prints could be obtained by the action of light, and to demonstrate how the solar camera might be employed to secure images in this manner. Niepce, Fox Talbot, and Daguerre followed these philosphers, and with the further aid of Sir John Herschel, Archer, and others, established photography as a practical art such as we now know it.

Of these names it has usually been the fashion to select those of Daguerre and Fox Talbot as being the most entitled to rank as the discoverers of photography; in many popular works, however, and especially those of foreign origin, Daguerre receives the lion's share of the honour for having in 1839 brought forward Daguerreotype, a process at once practical and efficient. Some have gone even further and have stated that Daguerreotype was the true germ of photography, and that before Daguerre made his important discovery no reliable results at all of light-printing had been obtained. Now, we do not for one instant wish to detract from the merits of Daguerreotype, which was, and indeed still is, one of the most beautiful and delicate methods we possess of reproducing images in the camera; but at the same time we must heartily protest against the same being considered the first step in photography. Fox Talbot's inventions, which date from the same period as that of Daguerre, although by no means so finished and complete

as the beautiful method of his French rival, have been far more fruitful in their subsequent career, and form assuredly the true basis of the processes of to-day. Among other discoveries, that of producing numberless positives from one negative may be cited as of exceeding importance, a circumstance which shows, moreover, that Talbotype is quite dissimilar from Daguerreotype (where only one image can be produced for each exposure in the camera), and that it is in principle identically the same as our present mode of proceeding. But we shall endeavour to show, and we hope also to prove most undeniably, that Niepce, whose name is generally but incidentally mentioned as a participator in the discovery, ranks, if not above, certainly upon an equal footing with Talbot and Daguerre as an early traveller along the road of photographic science. Indeed, if we take photography to mean simply the reproduction of an object or landscape in nature, caught up in the camera upon a sensitive screen upon which it remains after the action of light has ceased, we cannot but regard Nicephore Niepce as the first accomplished photographer, an inventor, moreover, who had attained his end, not by any stroke of good fortune, but as the result of patient and untiring investigation. Unfortunately for him, as in the case of many other inventors and discoverers, past and to come, he never reaped in any way the glory and advantages of his labours; but, on the contrary, has been during the past few years in a fair way of losing almost entirely the bare honour to which he is so justly entitled. Any contribution to the true history of so great an art-science as photography must, we feel sure, be acceptable to all students of science, and for this reason we make no apology for introducing to the readers of the "Quarterly Journal of Science" a few facts tending to place Niepce in a more favourable position than he has hitherto held as one only among many rivals.

Of the invention of Daguerreotype we know comparatively little. Daguerre himself never gave any explicit account of his early researches, with the exception of the manner in which the development of the latent image first became known. This, our readers will remember, was an accidental circumstance, and happened in this wise. After many fruitless exertions to secure an image upon a silver plate treated with iodine, he was examining on one occasion some of his plates, taken from a cupboard which served him in the double capacity of laboratory and store-room, when to his unbounded amazement he found a half-developed picture upon one of them. His surprise was the greater from the

fact that he was confident no such image existed upon the metallic surface at the time of its being experimented with, and that, therefore, to whatever cause the phenomenon was due, it must have occurred subsequent to the plate's entry into the closet. The mystery was hard to unravel, and Daguerre for some time was at a loss for a solution thereof. A second plate, prepared in the same manner and exposed to light in a camera, was put into the mysterious closet, and after a short sojourn therein the same startling result appeared. At last, hidden away in a corner of the cupboard, where it had long remained unheeded and forgotten, was found a bowl of mercury, the vapour of which condensed in a more or less concentrated form upon the isolated plate, and thus developed the latent image.

This was, perhaps, the first discovery of a photographic latent image;* but in no sense can it be considered the foundation of photography, and for two reasons; in the first place, as we shall presently see, Daguerre was anticipated by at least twenty years by Niepce in securing a photograph from nature in the camera; and, secondly, it is not Daguerreotype, but Talbotype, which is, as we have said, the basis of photography now-a-days. Daguerreotype was already a complete art when first made known by its discoverer, and could not, therefore, be improved upon to any marked extent, while Talbot's process, less elaborated possibly, was the stepping-stone to further progress. But it is not for the discussion of the relative merits of these two inventions that we have here taken up the pen, but rather to assert the claims of Nicephore Niepce, to whom we shall now turn without further ado.

M. Victor Fouque, a man well known in France, both in literature and science, has recently collected together and published the letters and papers of Niepce, and with these before us, a very clear estimate of that philosopher's doings may be formed. The letters are of an exceedingly interesting nature, and written for the most part to a brother between whom and the writer there existed a kind of partnership. Nicephore, it seems, had been brought up to the profession of a soldier; but after serving in the South of France and Sardinia for some years as sub-lieutenant, his health and failing eyesight obliged him to relinquish military life. His elder brother retiring about the same time from the French

* In all probability the honour of discovering the latent image belongs to the late Rev. J. B. Reade, F.R.S., who as early as 1837 developed by means of gallic acid some images of insects secured by means of the solar camera upon salted and silvered paper.

navy, the two brothers settled down together at the little town of St. Loup de Varennes, there to earn their bread in the best way they could. During the first years, being both of an ingenious nature, they turned their attention to mechanical propelling contrivances, and invented a description of velocipede, termed by them a pyreolophore, and this appeared to them so successful that Claude was despatched to England in 1807 for the purpose of selling the design. Left to himself, Nicephore continued some experiments he had been making with lithography, then a novelty brought forward by Senefelder, in Germany, and was seized with the idea of elaborating a plan by means of which a design reflected upon a stone would be permanently fixed upon the surface. This idea led to the institution of researches with the camera, so as to obtain the actual result given by the retina of the eye, and his exertions in this direction are detailed with much precision in a letter to his brother Claude, dated 12th of April, 1816, of which the following is a translation:—

"I have profited by the short time we remain here to construct a kind of artificial eye, which is simply a square box, measuring six inches each way, and furnished with a telescope-tube containing a lens. Without this apparatus it would be impossible for me to judge of the merits of my process."

Here we have a description of a camera with movable focus clearly described. On the 5th of May, Nicephore writes:

"You heard in my last letter that I had broken the lens of my camera, and that I had another one which I hoped to make use of. In this I was disappointed, for the focus of my other lens was shorter than the diameter of the box . . . . . As I could not use my camera on account of the broken lens, I constructed another artificial eye with a ring-box, measuring from sixteen to eighteen lines square. This was fitted with a lens from my solar microscope, which, as you know, formerly belonged to our grandfather Barrault, one of the glasses possessing precisely the requisite focus. By means of this little apparatus I obtained images most clearly and sharply defined, reflected upon a field thirteen lines in diameter. I placed my apparatus where I operate, in front of the pigeon-house, and made an experiment in the usual manner; I obtained on the white paper the whole of that portion of the pigeon-house which is seen from the window, and a faint image of the casement, which was less brilliantly lighted than the exterior objects . . . . I am well aware that there are many difficulties in my path—above

all, the problem of fixing the colours—but with steady perseverance and much patience I hope to succeed in working out my designs. That which you foretold has happened; the ground of the picture is black, and the images are white; that is, lighter than the ground. I believe a method of obtaining pictures by this means might be made use of, and I have seen engravings of a similar description. It may not be impossible to change the disposition of the colours, and I have made some experiments in this direction which I am anxious to verify."

From the above we see that Niepce had formed a very excellent idea as to how an image could be secured, and was truly in a fair way to solve the mystery of printing by light. At the same time he was aiming at far too much when he sought to secure the colours themselves, and not merely their impression upon some sensitive material. His progress in heliography was, however, extraordinarily rapid, for we find from a letter written on the 19th of May, 1816, that he had undoubtedly succeeded in securing an image of some kind in his tiny apparatus. He does not, however, seem to have been much struck with the wonderful nature of his results, for he alludes to them almost incidentally:—

"I hasten to reply to your letter of the 14th, which was received the day before yesterday, and with the contents of which we were much pleased. I write on half a sheet of paper only, as mass this morning, and a visit to be made this evening, leave me but little time; besides, I have to enclose two pictures taken by my process, and it is necessary, therefore, that I should not increase the weight of my letter too much. The smallest image was obtained in the ring-box, and the other from the medium-sized camera of which I spoke some time since. You will be able to judge of the effect better by placing yourself somewhat in the shade, and holding up the pictures placed upon an opaque body against the light. A picture of this description is, I believe, subject to alteration after a time, as although proof against the action of light, the reaction of the nitric acid in its composition will have the effect of destroying it; it is possible, also, that the prints may become damaged by shaking and jolting during their transport. This is, of course, merely an experiment, but if the materials were rather more sensitive (as I hope to obtain them), and, above all, if the order of the shadows were inverted, the illusion would, I think, be quite complete. The two pictures were made in my operating room, and the field is merely the

diameter of the casement. I read in the Abbé Nollet* that in order to reproduce a larger number of distant objects, a lens possessing a very long focus must be used, and a second glass must be fitted into the tube carrying the lens. If you desire to preserve the impressions, although they are scarcely worth the trouble, you have merely to place them between two sheets of grey paper and put them into a book. My future investigations will be conducted with a three-fold object—

"1. To obtain a clearer definition of the reflected image.

"2. To transpose the tints or shadows.

"3. To fix the tints, an operation which is by no means an easy one."

It is not definitely stated what is the nature of the exciting chemical employed, but from the hint given there can scarcely be a doubt of its being nitrate of silver. In any case, however, we have here absolute proof that Niepce, working alone in a small out-of-the-way provincial town, far from all scientific resources, and ignorant, may be, altogether of the preliminary researches of Wedgwood and Davy, made several years previously, succeeded in catching up and securing the images reflected in the camera. Of course, as we know very well, such images were not permanent, and required to be fixed before their durability for any lengthened period could be ensured; but, after all, it must be remembered that even at the present day the imperishability of silver prints is still an unsolved problem. But Niepce's success was not yet complete; from the very first it is apparent that he entertained a remarkably clear notion of what was necessary to elaborate a process of this nature, and the precise and orderly manner in which he arranged his ideas and conducted his research, catching hold of his subject as it were at the right end, could not but fail in bringing him to sound and speedy conclusions. After having succeeded so far as to hold fast the shadow, the next task he set himself to accomplish could scarcely have been better chosen as a sure means of progressing on his way. In his next letter, dated but nine days afterwards, he tells how fortunate he has been in his later efforts; and, indeed, this commuication reads more like the words of an enthusiastic amateur photographer of the present day, rather

* L'Abbé I. Antoine Nollet, Member of the Académie des Sciences; born 1700, died 1770. He occupied himself chiefly with the study of electricity, and was distinguished as an accomplished lecturer and exponent of the physical sciences. His principal work was entitled "Leçons de Physique Experimentale," in six volumes, published in 1743.

than those of a pioneer in science of some half century ago. He writes to his brother Claude in England:—

"I send you herewith four new prints—two large and two small ones—which I have obtained of sharper definition by the adoption of a very simple process, which consists in contracting by means of a disc of cardboard the diameter of the lens. The pigeon-house is reversed on the pictures, the barn (or rather the roof of the barn) being on the left instead of the right. The white mass which you perceive to the right of the pigeon-house, and which appears somewhat confused, is the reflection upon the paper of the pear tree, which is some distance farther off; and the black spot near the summit is an opening between the branches of the tree. The shadow on the right indicates the roof of the bake-house, which appears somewhat lower than it ought to be, because the cameras were placed about five feet above the floor. Finally, those little white lines marked above the roof of the barn are the reflection upon the sensitive film of some trees in the orchard. The effect would be more striking if, as I told you, the lights and shadows could be inverted. I shall confine myself to remedying this defect previously to endeavouring to fix the colours."

Another letter, written on the 2nd of June, contains further proof of the successful nature of the photographic results, in these words:—"The colour of the pigeon-house near the yard is of a brownish tint, but above the doorway, and near the pig-stye, there is a white patch, which will be found distinctly reproduced on the specimens."

We see, therefore, that beyond all doubt Niepce in 1816 produced a real photographic landscape, as perfect in truth, from a scientific point of view, as any of the negatives taken at the present day. But his researches did not by any means end here; Niepce knew almost as well as we do now what were the principal defects of his process, and laboured diligently to overcome them. Our limited space precludes us, unfortunately, from making any further extracts from these highly interesting letters, which form so perfect a history of his experiments; but within a month of the last communication we find our hero hard at work to discover some chemical agent capable of being bleached, upon which the solar rays might be impressed, so as to obtain a picture in its natural lights. One after another were all the then known compounds, that he thought likely to suit his purpose, experimented with, but none were found sufficiently sensitive for manipulation in the camera. Many, also, were the ingenious

devices attempted to promote the bleaching action of the light, supplies of carbonic acid, chlorine, and hydrogen, being at times introduced into the camera during the period of exposure, but all to no purpose, and Niepce thereupon wisely abandoned this hopeless problem, and addressed himself to the question of producing permanent photographs. In this he was more successful, albeit the research was a long and tedious one, for we find that in the years 1823 and 1824 he had obtained permanent pictures upon metal, glass, and paper surfaces, by employing as the sensitive agent bitumen of Judea, which is rendered by the action of light insoluble in certain essential oils that otherwise readily dissolve it. So permanent, indeed, are these early examples of light-printing, that there exist even at the present moment at the Museum at Chalons, as likewise at our own British Museum, several specimens of Niepceotype executed about this time.

We must pass rapidly over Niepce's further experiments to elaborate a photo-engraving process, the results of which still form the basis of several well-known photo-lithographic methods, as likewise his essays with silver plates and iodine, which seem to point so curiously to the subsequent invention of Daguerreotpye. Our readers probably remember the circumstance of Niepce's journey to London in 1827, to visit his dying brother at Kew (a photograph of Kew church taken about this time is, we believe, in the British Museum at the present moment), and how he afterwards remained in London in the hope of profiting by his invention; also the fact of his wishing to bring forward the results of his labours before the Royal Society, which body, however, refused to see his productions unless the whole history of the affair was divulged. After this period poor Niepce seems to have lost heart in his cherished undertaking, and returned the following year to France dissappointed and sadly broken down. It was on his way back through Paris that he first heard of Daguerre, then famous as a skilful painter, to whom he was introduced as one who had also worked in the same direction as himself. After some correspondence a deed of partnership was eventually drawn up between the two in December, 1829, from a perusal of which it would seem that Daguerre was by far the chief gainer in the contract; in this document, of which a *fac simile* has been published, Niepce describes in detail the whole of his experiments and results from first to last, while Daguerre discloses nothing whatever, but agrees simply to contribute his labours towards the further elabora-

tion of the process. The position of the parties is, indeed, very clearly explained at the commencement thus:—

"Art. 1. A partnership will be established between MM. Niepce and Daguerre for the purpose of working out the said discovery invented by M. Niepce, and improved by M. Daguerre."

We all know how Niepce died a few years after this agreement was made, while the world was yet unacquainted with the works he had so patiently and modestly accomplished, and how Daguerre in 1839 communicated to the Paris Academy the wonderful process of Daguerreotype, a method which, while dissimilar and superior to that of Niepce, was nevertheless due in some measure to that philosopher's discoveries. All honour and fame to Daguerre for making one mighty stride of progress in the beautiful art of photography, but we should doubtless have admired him more if, when publishing his invention to the world, instead of ignoring Niepce's early aid, he had generously made some mention of the labours of his dead partner.

II. PNEUMATIC TRANSMISSION.

By Frederic Charles Danvers, A.I.C.E.

THE practical application of air to the transmission of carriages on land dates only from the commencement of the present century. The first idea, however, of transmitting power to a distance by means of pneumatic pressure appears to have originated with the celebrated Denys Papin, a Frenchman, who, in 1688, described an apparatus in which a partial vacuum produced in a long tube, by air pumps fixed at one end, caused the motion of pistons at the other end; but no record remains to prove that any steps were taken by Papin to carry his suggestions into effect so as to derive any useful practical advantage from them. The introduction of the locomotive engine naturally directed the attention of engineers and others to the subject of the provision of improved means of communication, and this desire was doubtless stimulated by the acknowledged defects of the locomotive at that time, and its reputed inapplicability for lines with gradients exceeding 1 in 100. The first person to introduce the atmospheric system of propulsion was a mechanical engineer named George Medhurst, who, in 1810, published a pamphlet on the subject entitled "A New Method of Conveying Letters and Goods with

Great Certainty and Rapidity by Air," in which may be recognised the first practical suggestions for the introduction of what is now known as the "Pneumatic System;" and it is not a little surprising to find that in this, and two subsequent pamphlets by the same author, are foreshadowed almost everything that has hitherto been discovered in connection with this subject—all subsequent inventions having reference merely to the detailed means for carrying that system into effect.

George Medhurst's first idea clearly was to employ the pneumatic system for the conveyance of small parcels only, but he subsequently suggested its application for the transport of goods of a more bulky nature. It is perhaps a pity that he did not confine his attention in the first instance to the development of his earliest ideas on the subject, and which has subsequently been proved to be the most practical method of applying his inventions, viz., for the transmission of letters and small parcels. The rage of the day being, however, for improved means of communication, it is not surprising that his own ambition and the popular clamour should have caused Medhurst to endeavour to apply his invention to a purpose for which it was ill-suited. We shall not now follow the progress of the gradual rise and fall of the atmospheric railway, from the time when John Vallance, in the year 1826, constructed a model tunnel in Devonshire Place, Brighton, 120 feet long, and nearly 8 feet in diameter, through which a carriage was propelled by means of air pumps worked by two steam engines, which was the first of its kind ever constructed, to the abandonment of the atmospheric principle upon the Paris and St. Germains line for the last mile and a-half of its length, which was taken up in the year 1860, after having been in successful operation for about 15 years. From this last-named circumstance it is clear that the atmospheric system is not wholly unsuited for railways under certain circumstances, the chief ground of its applicability being upon very steep inclines, such as were unsuited for locomotives. The inconvenience, however, of having different systems of propulsion upon the same railway has been the cause which has led to the abandonment of the atmospheric principle upon every railway where it has ever been tried; the general advantages, greater speed, and undoubted superiority of the locomotive gaining for it, in every case, the preference over the latter.

Thus ended all attempts to introduce atmospheric railways; but a few years before their final abandonment the adaptation of the principle for the transmission of small

parcels was again revived, and it now seems likely to come into very general use, especially in connection with the Post Office and the Telegraphs. The want of some means of speedy communication between the offices of the Electric and International Telegraph Company in London, in addition to that afforded by their lines of wire, probably led to the invention of a pneumatic tube for that purpose by Mr. Latimer Clark, and the first tube was laid down by that company in 1855. This tube was of lead, and the carrier (in which messages were placed) nearly fitted the bore, and was covered with felt. It turned out to be a complete success, and a similar principle was adopted by the Prussian Telegraph Administration in Berlin, between the Telegraph Office and Exchange, in the year 1863; in 1866 it was also adopted in Paris in connection with the Electric Telegraph stations in that city. Later still the principle has been introduced into New York, and quite recently a line has been laid down by Messrs. Siemens, in connection with the General Post Office in London, for the conveyance of messages in original between Telegraph Street and Charing Cross and on to the House of Commons, instead of sending them by Telegraph.

There are two methods which have at different times been adopted for impelling carriers through pneumatic tubes, the one being by creating a vacuum in front of the carrier, which is then impelled forward by the atmospheric pressure with a force equal to the difference between the latter and the vacuum. The other is by creating a "plenum" behind the carrier, or, in other words, increasing the pressure behind the carrier beyond that of the atmosphere, the difference between these two forces in pounds or ounces per square inch representing the force expended in driving the carrier through the tube. Medhurst, in his first invention, adopted the latter principle, and the introduction of the vacuum for the purpose is ascribed to John Vallance, who proposed it in a pamphlet published by him in 1824. Experience has shown that the vacuum is the far preferable manner of working pneumatic tubes, and it is also more economical than working with a plenum.

The difference between the effects of compression and exhaustion would appear, so far as recorded experiments upon the subject show, to vary in the cases of tubes of different diameters; but as a general rule it has been observed that when a carrier is inserted into a tube it is driven forwards with a mean velocity corresponding to that with which the air at the higher pressure is introduced behind it, or that at the lower pressure is exhausted in front of it. In a paper read

before the British Association at Liverpool last year by Mr. Robert Sabine, that gentleman has worked out a number of formulæ for calculating the work performed in pneumatic tubes, and the result of his investigations on this subject cannot fail to be of great value, as it is one upon which very little of scientific value has hitherto been published. "The problem of a successful pneumatic system," says Sabine, "is simply this: To make a given quantity of air expand from one pressure to another in such a way as to return a fair equivalent of the work expended in compressing it. It is obviously impossible to regain the full equivalent of the work, because the compression is attended with the liberation of heat, which is dissipated and practically lost to us. Therefore, in designing a pneumatic system, that which we have to do is first to contrive means of compressing the air as economically as possible; secondly, to get back as much as we can of the mechanical effect stored up in our already compressed air, irrespectively of the work which was employed in compressing it. The utmost theoretical work which a given quantity of air can be made to perform is evidently that of expanding from the higher to lower pressure; and the mechanical effect employed in propelling a carrier and air through a given tube is therefore equivalent to that due to the expansion of a tubeful of air from the higher to the lower pressure." The speed at which a carrier travels in a horizontal tube has been worked out by Sabine, and is expressed by the following equation:—

$$s=\sqrt{2g\frac{vf-\mathrm{W}l\mu}{\mathrm{W}+\frac{w_1+w_2}{2}v\left(1+\zeta\frac{l}{d}\right)}}\text{ feet per second.}$$

But when going up or down an incline—

$$s=\sqrt{2g\frac{vf-\mathrm{W}l(\sin\alpha+\mu\cos\alpha)}{\mathrm{W}+\frac{w_1+w_2}{2}v\left(1+\zeta\frac{l}{d}\right)}}\text{ feet per second.}$$

In these equations the volume of the tube in cubic feet is represented by v; l represents the length of the tube in feet, and d its diameter, also in feet; W is the weight of the carrier in pounds, and g the accelerated motion due to gravity; f represents the mechanical effect performed by one cubic foot of air; μ, the coefficient of friction of motion of the carrier in the tube; w_1 the weight in pounds of one cubic foot of air at the higher pressure; w_2 the weight of a cubic foot at the lower pressure, and α the angle made by the tube with the horizon, and which is + when the carrier ascends, but − when it descends. ζ is an empirical constant;

experiments to determine its value have been made by Girard, D'Aubuisson, Buff, Pecqueur, and others, who give a mean value for it of 0·02.

Dr. P. Brix, Professor at the Bau-Akademie, has published in the German "Telegraph Journal" particulars of experiments made by him upon velocity with a tube 2½ inches in diameter, laid down some years ago, by Messrs. Siemens at Berlin, between the Exchange and Central Telegraph Station, the results of which were that when working with compression the tension of the air at either end of the tube was 19·31 lbs. and 14·75 lbs., and with exhaustion, 14·75 lbs. and 10·19 lbs., respectively; the mechanical effect produced by one cubic foot of air in each case was 512·17 lbs. in the former, and 520·44 lbs. in the latter; and the weight of the air at the two extreme tensions was, in compression, 0·1099 and 0·0753 foot-pounds, and in exhaustion 0·0752 and 0·0447 foot-pounds. For each case the frictional resistance of the carriers in the tube averaged 0·1 lb.; the length, 2920 feet for each half of the tube; its diameter, 0·193 feet; and its volume, 85·49 cubic feet. With these values, the formula worked out by Sabine gives the calculated speeds in these two experiments as follows:—With compression, 34·1 feet, and with exhaustion 43·2 feet per second, or that the carrier should have occupied in the transit from station to station, in the former case 86 seconds, and in the latter 68 seconds, differing from the observations made by Dr. Brix 9 seconds in the one case, and only 2 seconds in the other. This difference, Sabine thinks, may possibly be due to an error of observation of the pressure, or possibly to the fact that the constant ζ may not be the same for small welded iron tubes as for a large cast-iron tunnel.

Mr. Sabine has also made some experiments with the tube of the Pneumatic Company, between Euston Station and High Holborn, which was some years ago designed by, and carried out under the engineering superintendence of, Mr. Rammell and Mr. Latimer Clark. This tube is ⊓-shaped, 4½ feet broad and 4 feet high. The trains used were each made up of three trucks, and these were loaded with an average weight of 6 tons, making, with the carriages, a gross load of 9 tons. The average time occupied in running through the tube from Euston Station to Holborn was 7½ minutes, with a partial vacuum of 5 ozs. per square inch, whilst the empty trucks were returned to Euston Station with a compression of 5 ozs. per square inch in 6½ minutes. Assuming the temperature of the air to have been 20° C., and its mean pressure 14·75 lbs., it is calculated that in drawing

the loads through to Holborn the air was exhausted to 14·44 lbs., and in sending back the empty carriers it was compressed to 15·06 lbs. per square inch. From these data the mechanical effect due to the expansion of one cubic foot of air in the two experiments has been deduced to have been 31·964 foot-pounds with exhaustion, and 31·945 foot-pounds with compression.

In employing the same amount of mechanical effect, and the air remaining of the same mean specific gravity, Sabine finds "that the mean speed of transmission varies inversely with the length, and inversely also with the square root of the diameter of the tube. Thus, with an equal mechanical effect expended upon it in each case, a very light piston would travel through a tube of one mile long with exactly twice the speed with which it would travel through a similar tube two miles long. And further, if we had two tubes, each a mile long, one having a diameter of 4 feet and the other a diameter of 1 foot, the air in the larger tube would only travel half as fast as that in the smaller one, assuming, of course, the total work performed during the transit to be in each case equal. The cause of this is simply that the greater portion of the mechanical effect which in the larger tube is used for moving the greater mass of air, is, in the smaller one, converted into speed. If the case arose, therefore, that a pneumatic transit had to be made with a stated expenditure of work, we should proceed economically by adopting a tube of small rather than one of large sectional area. With an equal utilised engine power in each case, the mean speeds of transit of air through two tubes are inversely as the cube roots of their diameters and lengths. For instance, with a utilised effect of 10-horse power, the velocity of transit in a tube eight miles long, being 20 feet per second, that attainable with the same power in a one mile length of the same tube would be 40 feet, and if we had two tubes of equal length—one eight times the diameter of the other—the speed attained in the larger tube would be only half that attained in the former. To obtain the same speed of transit of a very light piston in two tubes of equal length and different diameters, other things being equal, the utilised horse-power must be directly proportioned to the diameter, whilst to produce the same mean speed of transit of very light pistons in tubes of equal diameter but different lengths, other things being equal, the utilised horse-powers of engines may be taken as directly proportional to the lengths. Similarly, when the lengths and diameters are equal, but the mean specific gravity of air

in the two operations are different, the mean speed of a very light piston being the same in its transits through the same tube, or through two tubes of equal dimensions, the utilised engine power is directly proportionate to the mean specific gravity of the air on the two sides of the piston. It follows from this, therefore, that in working by exhaustion less engine power is required, other things being equal, than in working through the same tube by means of compression. And it would also follow that in hot weather, and when the barometer is low, the working of a pneumatic tube should be less costly in engine power than in cold weather and when the barometer is high."

"With given utilised horse-power operating upon a given line, the velocity of a very light carrier would be reciprocally proportional to the cube root of the mean specific gravity of the air moving in it. Mr. Siemens has proposed to take advantage of this fact by the employment of hydrogen gas for propulsion in letter tubes instead of atmospheric air. The specific gravity of hydrogen is 0·07; that of air being 1. The speed attainable, therefore, by the substitution of this gas would be as—

$$1 \; : \; \frac{1}{\sqrt[3]{0{\cdot}07}}, \text{ or as 1 to } 2\tfrac{1}{2}, \text{ nearly.}$$

This plan would be easily practicable with Messrs. Siemens's system of complete circuit tubes, in which the same air is pumped round without being changed. With any of the ordinary systems by which the tube is open at one end, of course only the atmospheric air could be used in practice."

A by no means unimportant matter in connection with the working of pneumatic tubes is the mechanical means employed for producing the vacuum or plenum, as the case may be. Several methods have been introduced with this object. The first system adopted by Vallance in his model at Brighton, in 1828, was to produce the vacuum by means of air pumps worked by two steam engines, and this was the plan afterwards most generally employed on the several experimental lines of pneumatic railway laid down in different parts of the United Kingdom and elsewhere. In the Prussian pneumatic dispatch tube both compression and expansion are employed. The tube itself consists of two tubes of welded iron, 2½ inches in internal diameter, laid parallel to one another beneath the pavement. A transverse coupling connects them together at one end, whilst at the other end they terminate in two reservoirs, between which an air-pump

exhausts the air from one and compresses it into the other, thus keeping up a continual circuit of current within the tubes.

The pneumatic system in use in Paris differs from the foregoing principally in the use of water power instead of steam-engine power for working the tubes, each station being supplied with an arrangement for compressing air. Until recently the transmission of carriers between stations was effected by means of compression alone, produced by the action of water upon a chamber full of atmospheric air. This water is obtained from the River Ourcq, and is employed in the following manner:—Three wrought-iron cylindrical vessels are erected at each station, one of which is large and the other two smaller, and of the same size as one another. The larger vessel is connected by means of a pipe with the water mains of the town, and an exhaust pipe leads from the same vessel into the sewers; each of these pipes is fitted with a valve to enable the communication to be opened and closed at pleasure. From the top of this vessel a pipe leads into the first of the two smaller reservoirs, and these again are connected together by a pipe fitted with a cock, whilst the second smaller reservoir is in communication with the pneumatic transmitter. In order to obtain a supply of compressed air, the valve is closed in the tube communicating with the sewers, and that leading to the water mains is opened, allowing the supply water to rush into the larger vessel, and to displace the air which previously filled it. This displaced air is compressed into the two smaller reservoirs until the water has risen nearly up to the top of the larger vessel. A cock, in the pipe communicating between that and the smaller reservoirs, is then closed to prevent the air returning. The water is then run out of the larger vessel into the sewers, and it again becomes filled with air, which is in turn compressed into the reservoirs as before, as soon as it is required.

The Pneumatic Dispatch Company in London employ a pair of horizontal engines to drive a fan 22 feet in diameter, by means of which the air can either be exhausted from, or forced into, the tubes at pleasure.

The latest form of pneumatic tube is that recently laid down by Mr. Siemens, from the Post Office in St. Martins-le-Grand towards Westminster, and which will probably be extended, in course of time, in other directions. Several entirely new features have been introduced in the construction of this tube, which we shall describe more fully at some future time in our chronicles of engineering, when the system

has become more perfected than it is at present. One new feature which Mr. Siemens proposes to introduce is a novel kind of blower, which he has designed, to be worked direct by the steam from a boiler, without the intervention of machinery, and which will not only effect a considerable saving in the first cost of the requirements of each station, but it will also be much more economical in working and for maintenance.

The methods adopted at the different stations on any pneumatic line differ, of course, with the various methods employed for transmission, and it is in this that very great improvements have recently been introduced by Mr. Siemens. We have stated that in the Berlin pneumatic despatch system two tubes are placed parallel to each other, and connected together at one end by a transverse coupling, whilst at the other they terminate in two reservoirs, between which an air-pump exhausts the air from one and compresses it into the other, by which means a continual circuit is maintained, and provision is made for the dispatch of messages in either direction. The tube for transmitting a carrier from any station is connected with the pressure reservoir, beyond which connection it is continued at a slight incline in which are placed two cocks, at an interval equal to the length of the carrier. Beyond the second cock the tube is open at the top in the form of a trough, into which the carrier is first placed. The first cock is then opened, allowing it to pass down the tube as far as the second cock. The former is then closed again and the latter opened, whereupon the carrier descends the inclined tube until it passes the pipe communicating with the pressure reservoir, whereupon it is caught by the current of air and blown to its destination. These carriers are thin metal cylinders, nearly filling the tube, supported by four wheels, two at each end, and alternately at right angles to each other. The receptor consists of a square box placed in continuation of the tube connected with the exhaust reservoir, with which it communicates by means of a cock bored to the same diameter as the tube itself. This box is lined with brushes, through which the carrier forces its way, and its impetus is thus checked, whilst at the extreme end is an india-rubber buffer. The Exchange station, at the other end of the tube, is supplied with similar apparatus, only, of course, without the engine, pump, &c.; instead of which the tubes are connected together by a short coupling tube. In some experiments which were made to determine the relative pressures in the two reservoirs, it was found that with

equal differences of 9 inches of mercury above and below atmospheric pressure, the transit time of a carrier the whole distance of 3000 feet was 95 seconds from the station to the Exchange, whilst it only occupied 70 seconds in returning. The pressures now employed are, in the one reservoir, 7 inches of mercury over, and in the other 6 inches under atmospheric pressure. With this arrangement the transit times were, from station to Exchange 1 min. 30 sec.; and from Exchange to station 1 min. 20 sec.

In the system in use in Paris the same apparatus constitutes both the receptor and transmitter for the carriers. It consists of a cast-iron stand or pedestal, surmounted by an air-tight box, in front of which is a lid or door. Two tubes enter this box from opposite sides; one leading to the pressure reservoirs and communicating through a cock or valve with a branch below it with the box, whilst the other branch from the box is connected with a tube open to the air at the top, and also provided with a cock. A central vertical tube closed at the top is used when carriers arrive, and acts then as an air buffer, against which they expand their force. Beneath the box another tube leads to the next station. In sending a message it is placed in a box and the door shut, the cock communicating with the compression reservoirs is then opened, and the pressure of air blows the carrier through the tube. At this time the cock communicating with the open tube is kept closed, but when a message is to be received this cock is opened and the other kept closed—the open tube admitting the escape of the air in front of the carrier.

According to Mr. Siemens's new method, a complete circuit is formed by the current of transmission, with which several stations may be brought into communication with each other. The transmitting and receiving apparatus is extremely simple, and consists of two short pieces of tubing the same diameter as the main tube, and out of the latter a piece is removed of equal length. By means of a crank, or rocking shaft movement, either of these short tube pieces may be connected with the main tube by a simple movement of a lever, and thus brought into circuit. One of these short tubes is open throughout. This is the transmitter. It is ordinarily kept in circuit, so that messages to other stations beyond may pass through. When it is desired to send a message the circuit is broken by moving the transmitter a little to one side; the carrier with its message is then placed inside, and after communicating by signal with the station for which it is intended, the transmitting tube is once more

brought into circuit, when the current of air immediately catches the carrier and hurries it on to its destination. Upon receipt of the signal at the further station for which the message is intended, the person in charge brings the receiver into circuit. This consists of a tube similar to the transmitter, with the exception that it is partially closed at one end so as to catch the carrier as it arrives. Its arrival is ascertained by the click caused by its striking the partially closed end of the receiver, which is then drawn back to extract the carrier, and the open transmitter is at the same time thrown into circuit so as to allow any through messages to pass. As each through message passes a station it causes a small bell to ring, as a signal to the superintendent, and to enable him to count when any message is due for his station. By this means a continual service of messages may be carried on in any circuit, the amount of business transacted being limited only by the means of the superintendents at each station to keep pace with the arrival and dispatch of messages.

AN ESSAY TOWARDS ESTABLISHING A "SCIENCE OF MONEY."

By A London Merchant.

WHILE engaged with some views on the origin, nature, and use of money a few years ago, I happened to read Professor Müller's new work on the Science of Language; and from certain similarities apparent in the two cases, it then occurred to me, after his example, to entitle my subject the Science of Money. The Professor had satisfactorily established that language was not born of antecedent design, but rather of men's needs and capacities, in contact with external objects and events. As he had felt hesitation at the novelty of his term, much more had I at the unwonted apparition of a Science of Money. I did not therefore venture on the term on that occasion. Seeing, however, that the Science of Language is already a familiar phrase to us, I am encouraged to do so now. But I am still content with the introductory form of my title, hoping that on the next occasion with the subject I may at length venture to call it simply the Science of Money.

"As civilisation advanced," says Professor Price in his late lecture on Money, "an effective contrivance was

invented in coin, which every one consented voluntarily to take in exchange for the goods he had to sell, because he knew that when he himself required to buy he would be able to get other property of the same value as that he had sold for coin." If this be an accurate or probable account of the origin of our subject, there can be no Science of Money. Our chief question then is: Did money come into existence by antecedent design or otherwise?

To aid our illustration, let us take a few early historical data on the subject. They will present money to us in a simpler condition than at present, and in a state that may help us in tracing it still nearer its source. In early Greece iron was used as money; in early Rome rings of copper. Silver money came subsequently to Greece in her trading with Asia, along with the weights and measures and other commercial facilities of the teeming and busy East. Gold money came generally at a later time. The Greek trading communities—Argos, Ægina, Athens—readily adopted the silver; the interior and non-trading, as Sparta, clung longer to the iron. Lastly, the Lybians were reputed to have invented money—a question that will be estimated further on.

This rude money of early Europe, then, has developed into the vast and highly artistic money system of after times, concurrently with the great commerce of these times. But this advance has not been uniform over the world; and that world still presents to us even to-day, among Australian tribes, for instance, as well as other populations, a primitivism as free from industry in the commercial sense, from exchange of products and from money, as any that preceded Greece or Rome. We have ascended a step when, quitting our modern Australian, we come to Mr. Brookes's paterfamilias of Borneo, who, gathering a load of bees'-wax, perambulates the country till he has exchanged it for other things his household needs. But our Bornean showed readiness to advance when our countrymen, and others from without, introduced him to larger trading and the use of money. Commerce and civilisation seem born of such mutual intercourse. The steps of human progress are generally taken with a difference due to places and circumstances, and by intercourse these differences are mutually appropriated towards further progress. Civilisation, therefore, with its indispensable commerce, has chiefly flourished along the world's great streams of human intercourse, shaped, as these are perhaps chiefly, by physical and climatic features. The peoples who have remained outside, shunted from the active life of the world, have more or less preserved to us

the primitivism to which we can still to-day refer in illustration of our subject.

Let us now take a further step; let us, in short, go as near to the threshold of money as may still indubitably leave us outside of it. We shall suppose a society where labour is organised into a daily vocation; the society tolerably compact—not scattered and merely pastoral. As a whole it is fairly well off, with a permanent stock of means. In a society thus conditioned we should find that industry had taken the direction of the subdivision of labour. Each household would be busy over something special, instead of trying to produce directly everything it required. Each thus acquiring a special skill and knowledge in its own department, the productive power of society at large is greatly increased. Under this system, into which an industrious people, when sufficiently aggregated, instinctively adjust themselves, owing to practical appreciation of its advantage, there is concurrently, of course, a system of mutual exchange. These exchanges, which would assume at first only a casual and relatively unimportant aspect, will gradually by their universality come to be only second—if, indeed, second—to production itself.

Let us follow the probable course of the exchanges in such an early community. Each trader, we may suppose, will first supply his family from his own special wares, to the extent of their suitability and of his own means. He will next effect exchanges of his wares for his family's further wants; and lastly, he will aim at maintaining a stock adequate to his trade.

Now, if there were no diversities of personal business qualities, or such things as good and bad fortune, communities might plod along in this simple way for very lengthened terms without any development beyond the few and direct exchanges required. But surplus means gradually come into various hands, and when the immediate wants of the family and the trade are provided for, what is to be done with what is over—with a surplus always increasing in a vigorous and advancing society? We know that society does not relax its industry when entering on this phase, and therefore that a solution goes on concurrently from the first. The tendency will be to take opportunities of investing such surpluses in some value outside one's own trade. A clothier will not, as his productive power increases, pile up garments indefinitely, nor a butcher or baker mere additional heaps of his own kind of food. The surplus means will go to some variety of investment that commends

itself, in addition to the distinct advantage of variety, by superior keeping qualities, and especially by being an article readily realisable when the value, thus, as it were, safely but temporarily stored up, is wanted for some actual—some permanent—investment. In this way arises a system of intermediate or temporary holding of a kind of value that is considered most convenient and suitable. These instinctive efforts to provide for surpluses initiate the system of wholesale dealing. Practically the surpluses are offered at so much discount; and out of this dealing, as we shall see further on, emerge loanable capital and banking.

In a considerable aggregate of a market such as we are supposing, a good many different kinds of articles may at first seem, to the free fancy of the different traders, about equally suitable, on considerations special and general, for this temporary holding. But practical experience gradually dispenses with many of these as less generally suitable than others. The tendency is ever to a limitation of choice, and the action in this direction is the more decisive with the additional demand for these articles—their additional negociability, as the result of this extended business. In short, the drift of the market is towards some one kind of article, which commending itself previously by being in constant and general use, has now such a further market as to be the object of purchase and sale much more than any other, and thus to present quite a surpassing negociability. Let me add, from historical fact, that a society arrived at this commercial stage has always come into the use of the commoner metals—iron and bronze, or copper. These metals, in their wide-spread use and with their qualities in other respects, are so markedly superior to most, or indeed to all other articles alluded to, that one or other of them has generally come into the prominent position in question; and this position, we shall presently find, is no other than that of money.

Let us now turn to the early Greeks and realise how iron came to be used by them as money. Iron had come into general commercial use; it was the raw material of two chief trades, that of the fabrication of arms, and that of agricultural implement making. For both purposes the manufacture of the metal would go on in common up to a certain stage, whence there would be a divergence respectively to the sword and the ploughshare. Comprising at this stage the material of the two chief vocations, there would be relatively a great trade in the iron, which would consist most probably of bars conveniently portable and of moderate value, and as much alike as possible. Such a

kind of value, at once durable for keeping purposes and readily realisable, would be specially attractive to those having a surplus they were not otherwise prepared to invest. This practice become general, the bars are of course the subject of a much more extended market than before. Constantly bought and sold, not merely for the original manufacturing but for intermediate holding purposes, they thus come to be by far the most negociable of all articles.

A result of this practice, and one prompt to show itself, is that every other article comes to be valued in these iron bars. The needs of commerce will early jump at this obvious facility. Then, as every article has its value in bars, that indispensable basis of business—a price current of the market—becomes possible, because the bars are a common measure of value. The price current quickly makes its appearance. Further, the growing facilities of dealing in bars lead eventually to every exchange, as a rule, being effected by the medium of the bars. Both the facility and the accuracy of dealing in this way are so obvious that direct exchange or barter, as a rule of dealing at least, definitively dies out.

Let us realise clearly the position we have now reached. At first the bars were selected as a suitable kind of value for a temporary investment. This operation was simply one variety of the general barter system then going on. At last we find that exchanges generally, not merely those for a distinctly purposed although temporary holding, are effected in the first instance for bars. In fact, as the practical facility from accurate valuation and ready dealing gives a most decided superiority to this mode, seemingly round-about although it be.

Everything thus comes to be bought or sold in bars, whether for immediate exchange or upon a time contract. Some traders might still barter direct or bargain in kind. They might, for instance, exchange so much wheat for a sheep, or deliver at once a ton of lead in consideration of a ton and something more of the same three months hence. Most persons, however, will gradually prefer to contract value for value, which they are enabled to do by contracting for payment in bars. Bars are the most accurate and indisputable value, as well as the most realisable of all articles. Eventually, the custom is universal, at least in any considerable market. All bargains, then, as a rule being for bars, every trader has his stock of bars to accurately balance as well as otherwise facilitate his dealings. Lastly comes a trader who deals only in bars, and who yet has no connection with the special manufacturers the bars were originally

intended for. He receives balances of bars from the other traders for safe keeping until wanted; and, as this gives him an average available stock of them on hand, he can lend the bars to others, or discount the time-contract notes expressed in bars. This trader is no other than a banker—the rude prototype, indeed, of the dignified personage of to-day, who deals only in a highly artistic value derived from the subsequently distinguished "precious metals," or in his own not less artistic bank-notes. But our original is none the less a banker, and none the less is he dealing with money, which has now come into veritable existence in the community.

We of to-day, indulging in the facilities of *ex post facto* views, can perceive that, rude as the materials are to our highly-educated sense of money, these bars are nevertheless being used as money, for they have evidently a sphere of use of their own that is quite distinct from and independent of their original use for manufacture. Indeed, so evident is this to us, that we are apt to take for granted that our early community see it also. But this by no means follows. We must bear in mind that they have had no help whatever to such an idea, because the bars are still in the form in which they go direct to the forge or the factory. No doubt, some reflecting banker of that time may have noticed that the stock of bars in existence seemed greatly more than the manufacturer could want. But it must also strike him that the whole stock goes eventually, sooner or later, to the factory. "To-day in the till, to-morrow in the furnace," might be his concluding reflection as he dismissed this part of the question. Like some theorist of our own time who wondered at the huge stocks of modern trade, he would simply conclude that somehow the modes of business required them all. I do not hesitate, therefore, to assert that the probabilities are decisively against the view that at this stage money was detected, even after it had come into full existence.

But the banker, although he has as yet failed to perceive money, can from his vantage ground see clearly some of its effects: he sees that these bars repeatedly come back upon his hands. Perhaps out of mere curiosity on this point he has marked a number of them; and although he learns, as of course, that some of these marks have yielded up a brief life at the factory, yet others, probably much the larger proportion, go for some time at least to and fro between himself and the other traders. A convenience therefore suggests itself to his mind: he doubles the bar into a ring for the

sake of easier carriage. Hesitatingly he does so, because he has no other idea as to the bars than that of their use in "The Arts," as we would now phrase it. Every article, in fact, still belongs to the arts; and we can best realise our early banker's case by supposing some modern ironmonger to be bent on lightening work to his shopmen by doubling up some portion of a large stock of brass rods or iron crowbars, which he expected to remain long on hand, and whose frequent shifting he wished to facilitate. Even with more hesitation might the customers of our innovating banker receive these altered bars, which, with the sole idea of the manufacturer before them also, they would regard at first rather as things tampered with and depreciated than as made more convenient to their business. But the edge of the novelty gone, habit and convenience would steadily plead for the rings, and eventually they would penetrate everywhere by preference, except to the manufacturer's forge. Thus there will at last be two quite distinct articles—the bars and the rings, and it can now be seen that they have respectively quite different uses.

We have already traced the probable origin of iron money in Greece, and we have just seen how most likely arose the copper-ring money of early Rome. Thus we reach a further and most important stage of our subject, for money now stands forth confessed, instead of being indiscriminately mixed up in the general market. It is still, indeed, as much as before, one among the articles of that market; its use is still as before for temporary or intermediate holding; but having now its own distinctive pattern, the mind can now follow it into its own distinctive function. In short, although money had previously come into actual existence, it has only now come into differentially visible being.

Money thus distinctively standing forth, one early consequence is a distinctive name. A new idea occupies the mind, and a new, or at least a distinctively applied, name will be forthcoming for its recognition. This completes the separation of the rings from the bars, or, in more customary language, of money from merchandise. Let us attentively follow the further effects. At the bar stage, it will be remembered, we had, equally as at the ring stage, the reality of money, only that money had not become distinctively visible—had not, as it were, emerged from merchandise. We have seen that, even at the earlier stage, exchanges, as a rule, had come to be for bars, and all values to be expressed in bars; but that the mind had not parted, in all this, from the actual market of commodities. The bars

themselves were undistinguishably a portion of these commodities, used for convenience to value and exchange the rest. But now that money has shown itself and has acquired its own special name—money let us call it—there arises in the mind a new and irresistible tendency. A spade is no longer a spade. All dealings have been in bars or rings; now they are in money. What had been a loan of bars, or of other wares, repayable in bars, with some few additional bars as consideration, is now, with all attractive simplicity, a loan of money, repayable with interest. Business in this way is indeed marvellously simplified, and hence the irresistible tendency; but the popular mind has drifted into a misconception, from which, with every successive step of departure from the plain-speaking age of the bars, it is less and less able to free itself. Everywhere money crowds up to the business surface, obscuring the view of everything else in the market. As the banker turns over countless contracts, he meets with nothing but money. No doubt the consideration for all these engagements has been as a rule merchandise of some sort, and he is not and cannot be ignorant of that fact; but as the consideration is all expressed in money, the banker has nothing before him but money; while all this floating value that is so represented is to him "the money market." When his customers have large balances of this value, money is plentiful; when otherwise, money is scarce; and the consideration for the loan or use of the value is small or great accordingly.

But what is the Money Market, and what are those balances of value the banker is dealing with? They are simply those surplus means that we have seen gradually arising in society, and compelling their owners, by the force of their own interests, into that course of advantageous investment that has thus resulted in the use of a "currency," a "circulating medium," a "common measure of value"—in short, in the use of money. Our great modern money markets are mainly the result of the increasing productiveness of labour in its further and further adaptive subdivision in the home and foreign trade, with all the exchange system of that trade, and the huge stocks required to conduct it effectively. The Money Market is all this product, including such a proportion of money—usually but a small proportion—as the modes of business and the means and money-carrying habits of society happen to require.

Let me notice here in passing that the confusion here alluded to in the popular mind has given us two quite different meanings of the term "value of money." The value

of money, as of any other commercial article, should mean the exchangeable value, that value, namely, which places gold above an equal quantity of silver, and silver above as much lead or copper. But this proper sense of the term is almost lost sight of in the general prevalence of quite another sense, which, erroneously as we have seen, associates it with rates of interest and discount. Interest and discount refer to surplus means generally, to the capital of the community, or, more strictly, to the floating or circulating portion of the capital—the part of it that people have to spare, and that is thus available for loaning.

And now we return to our first and principal question, upon which we are, I hope, in a position to decide—Did money originate by design or otherwise? But the question has been already virtually answered when we found reason to conclude that even after money had come into being through the natural tendencies or "drift of the market," it had not been recognised, even in the act of using it, until the subsequent accidents of its further course had given it a distinctive unmistakable aspect of its own. Of course, therefore, it did not owe this existence to anticipation and design.

Nevertheless, there is so strong a general prejudice towards the opposite view that it may well be asked—What has caused and what sustains such an opinion? This bias has arisen chiefly, I think, from an exaggerated idea of the difficulties of early trade, under the barter system, prior to the use of money—difficulties so great, as we are apt to suppose, that our predecessors in trade must have been impelled into the invention of money. Indeed, volumes have been written upon the unbearable inconveniencies and obstacles of these pre-money times. But these difficulties are entirely of our own imagining, because, in the effort to realise them, our minds are prepossessed by a condition and scale of trade that had no existence in the times in question. The true case is, that each successive age has found its own facilities for all it had to do, and was no more under a sense of inconvenience than any people of to-day less advanced than ourselves, and with wants as limited as their commerce. Even the bees'-wax bartering of our Borneo Dyak would not have ceased upon a mere explanation of the use of money, unaccompanied by the additional commerce that required money's facilities. Money could not live in his thin atmosphere of trade, any more than double-entry book-keeping in the simple dealings of a costermonger. In some of our more sequestered villages, if, indeed, any such still remain in this railway age, the use of money even now may hardly be

considered general—a state of things that we, with our modern prepossessions, are apt to associate, although not always correctly, with the extremity of poverty. Money was not an invention, but a growth—a development arising gradually out of the needs of society. We have seen how it arose out of the Drift of the Market, and therefore there is a Science of Money.

Here my subject properly ends, because the further history of money has reference to the adaptations of an intelligent design—adaptations which, so soon as there is a clear view of money's existence and uses, are all more or less obvious —more or less readily seen to be advantageous or necessary. But in order to complete our question it is necessary still to follow the principal of these changes, because to most of us of to-day money has become almost indissolubly associated with the Mint and the precious metals. We have hardly even alluded to this part of the money question, and therefore to most people's ideas we have not even yet reached the recognisable domain of money. From our stage of the primitive bars and rings, therefore, let us, in deference to popular prepossessions, descend for a brief space into the modern arena.

The intervention of the State is at no great distance after the recognition of money. At first this may be limited to the mere guarantee by an official stamp of an article so generally used and trusted in; but eventually the State appropriates the whole question. A minuter subdivision of value is, no doubt, the very first of the secondary steps of money's subsequent progress. The ring—holding still to our representative and very illustrative ring—is broken into pieces to suit the wants of small dealings. For the opposite reason, there is next felt the want of the expression of high value in reasonable bulk, and silver and gold are successively brought in to supply the need. Gradually the monetary idea has become a subject so familiar and plastic that the State can venture on materials for its money without reference to any other consideration than their special suitability for money, and the cost of getting them. In this crisis the iron is liable to be summarily dismissed from office. Money takes to itself a further distinctive existence, and the materials of which it is chiefly composed are specially honoured as "the precious metals." But long prior to this result—perhaps concurrently with even the earliest of these various secondary steps—the artistic idea has been struggling to the front. It has never, indeed, been relatively behind in the early history of those intelligent races among whom money

has first appeared; and it finds its expression here in a shapely form given to the metallic pieces. In short, the rude fragments of the ring become coins, and the ambitious taste of a ruler is not long behind with the addition of his name and effigy. Such must be the true gage and the true importance, or rather unimportance, of the Libyan discovery.

Conventionality is now supremely enthroned in the domain of money. It is readily apprehended that money is not wanted in any sense of actual consumption applicable to other articles. While, therefore, a half ration of food could not be reasonably issued as a whole ration, yet adequate authority might command a half pound of money to circulate as a whole pound. Hence we have mintage and seigniorage rates, and depreciations and debasements of currency in all degrees. Finally, paper money dispenses with the cost of any value whatever. In its usual and regular form, paper money is the promise to pay the actual money whenever demanded; and where the issuing party is fully trustworthy, the sign is as good as the thing signified, and often a greatly more convenient instrument. To this "convertible paper" the inconvertible is only the too evident alternative.

These are all contrivances for supplying money below the cost to the issuing party. Many quarters may be simultaneously at work with these cheap contrivances, the banker as well as the government, and the forger, too, in his smaller, but not less economical, way. Can all this go on indefinitely, and has money at last emancipated itself from those laws of supply and demand that apply to other articles, and that evidently applied to money also as one of these articles before all this artificiality was introduced? Has money in short become a thing "*sui generis?*" In no wise whatever. Society's modes of business, and the individual habits as to money-carrying, settle themselves into the need of a certain amount of money, in whatever of money's forms this supply may come; and if this supply is exceeded, there is simply the effect due to a relative excess in the supply of anything else in the world of trade materials. The excess is returned to that world of materials whence it came, and if through seigniorage or other obstacles it cannot be so got rid of, the money is depreciated until it can.

We know that money has not in all cases arisen and been developed in the way here stated, namely, by undesigned tendency out of the most commonly used metals, and afterwards by conventional transfer to higher-value

metals. There is allusion in early history, for instance, to live stock being used as money. In modern times the early Virginian settlers used tobacco for the like purpose, and the eastern Asiatics still use brick tea, and the Abyssinians cakes of salt. And again we have the notable case—solecism among exceptions we may call it—of the cowrie money. But these exceptional moneys are always the product of exceptional circumstances. They arise, excluding for the moment the special case of the cowries, only in thin, scattered, or merely pastoral communities. Such money could find no life in any ordinary aggregate of intelligent industry, with its conspicuous factories of the common metals, and even, failing them, of many other articles more suitable for a temporary holding and for exchange purposes, than a sheep or a piece of tobacco. The drift of the market, as I have termed it, would be peremptory, decisive in this respect. Where there is no market in this sense of industrial human aggregation, this regular effect is not of course forthcoming. But all these articles of irregular money, so to call them, have ever had the main requisite of a general appreciation and use as articles of trade, giving the users the idea of realisableness or ready negociability, and thus meeting for each person the convenience of the hour or of the special case in circumstances probably of very limited choice. Such money, however, has generally at best held but a locally partial or a divided sway; and it is only in some secondary sense of this kind that we can interpret the old classic allusions to sheep and cattle that have come down to us. Cowrie money stands on a different ground. Its origin as money is doubtless attributable to the fancies of children, protracted into the later life of simple peoples, whose minds never rise greatly beyond those of children. The case is as though our schoolboys had preserved into later years their strongly impressed notions of the exchangeable value of their marbles, so that this money of boyhood's transactions had passed into those of mature life.

Lastly, let me advert to yet another subject, fraught, like some of the preceding, with popular misconception. I might entitle this closing paragraph, "the cost of a currency." People have not usually thought of money, indispensable as all admit it to be, as none the less a costly instrument to society. We have seen, however, that the tendency in each individual is to economise money—to find his advantage in doing with as little of it as possible; and hence the whole community is gainer by this course.

Countries the most commercially advanced have the greatest facilities in this respect; and hence arises the sort of popular paradox just hinted at, namely, that such countries possess, relatively to the amount of business done, the least amount of money. In other words, countries which, as having the greatest commerce, are the richest, require, and do actually possess relatively, the least money. In ancient times when there was comparatively little of commercial credit and the modern banking facilities, there must always have been a large amount of money relatively to the other wealth of society. We conclude that such a condition was not to the enrichment but the impoverishment of society, much as any other of its indispensable habits would be if gone about after a like costly fashion.

Money may be aptly compared to a road through a field. The field at first, we will suppose, showed no tracks, as every traveller in the business of passing through was finding his own. By-and-bye some definite paths appear, and eventually one or other of these becomes the common highway, broad and well-beaten, because used by every one. The passengers' object or design had not been to make a road, but to get through the field; nevertheless the road is the result. The road we perceive, indeed, to be indispensable for the object in view, but the illustration may help us to apprehend as readily, that the less of the field we must surrender to the road, the more of it will remain to us for other uses.

"WHERE ARE THE BONES OF THE MEN WHO MADE THE UNPOLISHED FLINT IMPLEMENTS?"

By WILLIAM PENGELLY, F.R.S., F.G.S.

WHY don't you find the *bones* of the men, as well as their *implements?* is a rejoiner constantly made, and with an unmistakable air of triumph, by those who hear with disbelief the statement that human implements have been found with remains of extinct animals, in cavern deposits and river gravels. It is unnecessary to say that the question thus put is based on the following assumptions:—

1st. That, under all conditions, the bones of man are as conservable as those of other mammals.

2nd. That no portion of the human skeleton has ever been found in association with the extinct cave animals.

3rd. That until they have been so found, the doctrine of "Man amongst the Mammoths" remains non-proven.

We purpose devoting this paper to a consideration of these assumptions, and in the order in which they stand.

I. The proposition that, "under all conditions, the bones of man are as conservable as those of other mammals," assumes a very plausible aspect, no doubt, when we are told that their composition is the same; that in Egypt no difference is observed between the condition of the mummies of men and those of quadrupeds; that, on ancient battle-fields, the bones of the warrior and of his horse are equally well preserved; and that Cuvier, who gave much attention to the question, did not believe in the contemporaneity of man with the mammoth.

Let it be supposed to be the fact that, in a general way, the bones of man and the lower mammals *have* the same composition. Since as much may be said in the case of plants, it might be thence argued that had there been in the old Carboniferous Period the same variety of plants as we have in the present day, representatives of all of them would have occurred as fossils in the coal and associated beds. The fact, however, is otherwise; for certain groups of the vegetable kingdom are totally unrepresented in the beds in question; and hence, if the argument is worth anything, they could have formed no part of the flora of the coal period. To this, however, it is sufficient to reply that in March, 1833, the late Dr. Lindley placed in water, in a tank, 177 specimens of various plants belonging to all the more remarkable natural orders, including representatives of all those which are constantly present in the coal measures, and also those which are universally absent. The uncovered vessel was exposed to the air, and left untouched, further than filling it up as the water evaporated, until April, 1835, or upwards of two years. At the end of that time it was found that certain kinds had entirely disappeared; others had left some more or less recognisable traces; whilst others —especially fungi, ferns, and coniferous trees—were comparatively well preserved. In short, the plants remaining and the plants which had disappeared were respectively of the same groups as those which are and those which are not present amongst the coal fossils. *

Again, it is well known that certain shells, such as oysters and limpets, are more frequently met with in a fossil state than others are, such as cockles. Now, both groups are

* See Lindley and Hutton's Fossil Flora, vol. iii., pp. 4—12.

formed of the same materials—carbonate of lime and animal matter. If, therefore, the argument of similarity of chemical composition is to be trusted, it follows that a rock rich in fossil oysters but poor in fossil cockles must have been formed at a time when, or in a locality where, the former abounded, but the latter were scarce. In 1862, however, Mr. Sorby brought the subject under the notice of the British Association, and pointed out that the carbonate of lime in the shells of limpets, oysters, and some other mollusks, took the form or condition of *calcite*, but in cockle shells and their allies, the form of *arragonite;* the difference being one, not of chemical composition but of physical arrangement, whereby the molecules were in stable equilibrium in the former, but unstable in the latter, thus giving the arragonite shells a liability to disappear from which those of calcite were exempt.*

It is obvious, therefore, that as it is unsafe to assert that a deposit poor in arragonite shells was formed at a time when, or in an area where, few such shells existed, and as it is not necessary to suppose that there were no lichens, or mosses, or grasses, or sedges in a given period of the past, simply because its deposits contain no fossils of these groups, it may be very far from certain that there were no men during what is called the cavern era, even if it be a fact that no human bones have ever been found in the "cave-earth."

Whilst it is true that the bones of the human and of the infra-human mammals consist of the same materials, it must not be supposed that the different bones of even the same animal, or different parts of one and the same bone, or corresponding bones in individuals of different species, are *identical* in composition. They differ in the proportions in which the components are mixed; or, in the language of the chemist, though they agree *qualitatively*, they differ *quantitatively*. We must refer our readers desirous of full information on this point to the analyses of Von Bibra,† from which we have compiled the annexed table (see next page), showing the composition of the thigh bone in the human subject at different ages, and in different species of the lower mammals.

Though it may be true that in Egypt no difference is observed between the condition of the mummies of men and of quadrupeds, it does not appear that the fact has much, if any, bearing on the question before us, for the human bones we are supposed to be in quest of could not have been under conditions at all similar. They were neither placed

* See Report of British Association, 1862; Proc. of Sections, p. 95.
† See Day's Simon's Chemistry, vol. ii., Pt. ii., pp. 396 *et seq.*

Table showing the composition of the thigh bone in the human subject at different ages, and in different species of the lower mammals :—

	Phosphate of lime, with a little fluoride of calcium.	Carbonate of lime.	Phosphate of magnesia.	Salts.	Cartilage.	Fat.
Human, 5 years	59·96	5·91	1·24	0·69	31·28	0·92
„ 19 „	54·78	10·90	1·34	0·83	31·15	1·00
„ 25 „	57·02	8·92	1·70	0·61	29·58	2·00
„ 25 or 30 years	59·63	7·33	1·32	0·69	27·70	1·33
Horse, 6 „	54·37	12·00	1·83	0·70	27·99	3·11
„ 14 „	54·63	11·28	1·50	0·40	27·98	4·21
Boar	58·88	9·02	1·17	0·92	28·00	2·01
Bull	54·07	12·71	1·42	0·82	29·09	1·91
Goat	55·94	12·18	1·00	0·50	29·68	0·70
Hare	58·45	9·07	0·99	0·82	29·60	1·07
Squirrel . . .	57·03	10·45	1·36	0·90	29·46	0·80
Rat	60·38	6·72	1·91	0·91	28·98	1·10

in well-formed tombs, nor swathed in protective envelopes; nor had the countries in which it is said they should be forthcoming the dry atmosphere of Egypt. They must have been buried by the action of water in a deposit which, in all probability, would constantly allow water to have access to them, in at least small quantities.

We have seen it stated that on some ancient battle-fields, in South America in particular, bones of men and of their horses have been found equally well preserved, and have no intention of calling it in question; but, before its bearing on the present subject can be admitted, we must know something about the atmospheric conditions of the districts in which the phenomenon has been observed.

The opinion of Cuvier, or any one else, on the question of the contemporaneity of man with the mammoth, is of much less importance to us than the facts and reasons on which the opinion was based. Authority is but little likely in the present day to be admitted in a scientific argument. Be this as it may, the large body of fact which has been carefully collected since Cuvier's time may be believed to be well calculated to have changed his opinion, as it has that of many eminent men of science now living. Thus M. A. de Quatrefages, Member of the Institute of France, speaking

on this point, says, "I should state that, after having for a long time shared the belief of Cuvier, I have arrived at a contrary opinion." *

It is much to be desired that some one would do for bones, including those of man, what Dr. Lindley did for plants, and ascertain by experiment how long-continued immersion in water would affect them. In the meantime, however, we are not without reason for believing that human bones soon disappear when thus exposed. In 1853 the Dutch Government succeeded in converting the Lake of Haarlem into dry land, and thus added 45,000 acres to the soil of Holland. There had been many shipwrecks and naval fights on this sheet of water; from thirty to forty thousand souls had their homes on its borders, and hundreds of men had found a watery grave in it. The canals and trenches dug to a considerable depth through the rescued land must have had an aggregate length of thousands of miles; yet not a single human bone was exhumed from first to last. Some arms, a few coins, and one or two wrecked vessels alone rewarded the antiquaries, who watched the operations in the hope of a rich harvest. Here, as in cavern deposits and river gravels generally, works of art alone furnished evidence of the existence of man, even though no part of the deposit could be more than three hundred years old, as the lake was formed by an inundation towards the end of the sixteenth century.†

II. If it be true that no portion of the human skeleton has ever been found in association with the extinct cave animals, the literature of the question bristles with unmistakable and grave errors, as we shall now proceed to show:—

In 1824, the Rev. Dr. Fleming published a paper, in which he objected, apparently on good grounds, to some of the speculations of Dr. Buckland, in his "Reliquiæ Diluvianæ, published the year before, and distinctly stated that "Man was an inhabitant of this country at the time these animals, now extinct, flourished, his bones and his instruments having been found in similar situations with their remains.‡

Mr. W. L. Wrey, F.G.S., who during many years resided in South Wales, between Llanelly and Llandeilo, recently informed us that in 1831 he heard of the discovery of a cavern in his immediate vicinity, in the ordinary course of

* See Anthropological Review, vol. i., p. 333, 1863.

† See Lyell's Antiquity of Man, p. 147 *et seq.*

‡ See "Remarks illustrative of the Influence of Society on the Distribution of British Animals," in the Edinburgh Philosophical Journal, vol. xi., pp. 287—305.

quarrying the lime-rock. On visiting it he found the roof beautifully studded with stalactites, whilst the floor was a thick stratum of stalagmite extending from side to side. This floor rested on a mass of dark red very tenacious clay, throughout which bones of extinct and recent animals were mixed indiscriminately. Mr. Wrey obtained a large number of these remains, including an undoubted human skull, very perfect and in good preservation, found with the other relics. Believing the case to be unique and important, he at once wrote to Dr. Buckland, who lost no time in repairing to the spot, where he spent three days as Mr. Wrey's guest. For the explication of the facts, Dr. Buckland suggested that a human body had been buried in the clay, and thus the remains of man and of the much older extinct mammals had become mixed, after which the sheet of stalagmite had been formed over the whole. "This hypothesis," said Mr. Wrey, "was never satisfactory to my mind, because the human bones should have been lying together in the form of a skeleton, but instead of this they were mixed through the clay with the other bones." Many of the remains, including the human skull, were presented to Dr. Buckland, who expressed his intention of placing them in the Museum at Oxford.

The late Dr. Schmerling, of Liège, having carried on extensive researches in the numerous caverns in the Valley of the Meuse, published the results of his labours in 1833-4,* and stated that the deposits in many of the caverns were covered with a floor of unbroken stalagmite, and contained the commingled remains of extinct and recent animals, including man; that the human relics were of the same colour and in the same condition as those of the lower animals; and that they were so rolled and scattered as to show that they were not intentionally buried there.

Amongst the human relics found by Dr. Schmerling were several skulls, the most perfect of which, known as the Engis skull, from the cavern in which it was found, has attracted much attention, and according to Professor Huxley, is "a fair average human skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage."†

In 1840 Mr. Godwin-Austen read to the Geological Society of London a paper on "The Bone Caves of Devonshire," which, with other papers on the same district, was published

* Recherches sur les Ossemens Fossiles Découverts dans les Cavernes de la Province de Liège. See also Lyell's Antiquity of Man, pp. 63—74.

† Man's Place in Nature, p. 156.

in 1842 under the general title of "The Geology of the South-East of Devonshire."* In his description of Kent's Cavern, Torquay, he says, "Few, I imagine, who are acquainted with the facts which the labours of MM. Schmerling, Marcel de Serres, and others have established, entertain any doubts as to the fact that the bones of man have been found in caves; what I wish to state distinctly is, that they occur in Kent's Cavern under precisely the same conditions as the bones of all the other animals. The value of such a statement must rest on the care with which a collector may have explored; I must, therefore, state that my own researches were constantly conducted in parts of the cave which had never been disturbed, and in every instance the bones were procured from beneath a thick covering of stalagmite; so far, then, the bones and works of man must have been introduced into the cave before the flooring of stalagmite had been formed. It may be suggested that this cave was used as a place of sepulture by some early inhabitants of this country, and that bones of the other animals occupied the lower parts of the cave when such sepulture took place.

"In this case our researches should expose the human skeletons entire, as in the Paviland Cave; or at least the bones should occur in some sort of mutual relation to each other, but no such thing has ever been observed by any explorer in Kent's Hole; so that as far as the evidence from this case is to be our guide, ... there is no ground why we should separate man from that period, and those accidents, when and by which the cave was filled."†

In a discussion at the Plymouth meeting of the British Association, in July, 1841, the same geologist, according to the "Athenæum,"‡ said, "at Kent's Hole, near Torquay, arrows and knives of flint, with human bones, in the same condition as the elephant and other bones, were found in an undisturbed bed of clay, covered by 9 feet of stalagmite." Dr. Buckland, in reply, "contended that human remains had never been found under such circumstances as to prove their contemporaneous existence with the hyænas and bears of the caverns. In Kent's Hole the Celtic knives and human bones were found in holes *dug by art*, and which had disturbed the floor of the cave and the bones below it."

Dr. Buckland's reply is chiefly valuable as showing that Mr. Austen was correctly reported. The "holes dug by

* Trans. Geol. Soc., 2nd Ser., vol. vi., part 2, pp. 433—489.
† *Op. cit.*, p. 446.
‡ Athenæum, Aug. 14, 1841, p. 626.

art" in Kent's Cavern, of which the former spoke, were purely hypothetical, and invented solely in defence of a foregone conclusion. Dr. Buckland's knowledge of the phenomena of the cavern was entirely derived from the researches of other persons, chiefly the Rev. Mr. MacEnery, who spent several years in the exploration of Kent's Hole, and declined the acceptance of the hypothesis, as the following passage shows:—"Dr. Buckland is inclined to attribute these flints to a more modern date by supposing that the ancient Britons had scooped out ovens in the stalagmite, and that through them the knives got admission to the diluvium. . . . . Without stopping to dwell on the difficulty of ripping up a solid floor, which, notwithstanding the advantage of undermining and the exposure of its edges, still defies all our efforts, though commanding the apparatus of the quarry, I am bold to say that in no instance have I discovered evidence of breaches or ovens in the floor, but one continuous plate of stalagmite diffused uniformly over the loam."*

The late Colonel Hamilton Smith devoted a section of his "National History of the Human Species" (1848), to the question of "Bones of Man among Organic Remains," of which the following is a brief summary:—In a conversation with the author in 1824, Cuvier admitted that the opinions then in vogue on the point would require considerable modification. Donati, Germer, Rasoumouski, and Guetard, maintained that human bones had been found intermixed with those of lost species of mammals in several places; they had been detected in England in caves and fissures; they were found at Meissen in Saxony, and at Durford in France, by M. Firmas. A fossilised skeleton found in the schist at Quebec, and in part preserved at the seminary, excited no attention; and the well-known Guadaloupe skeletons had been pronounced recent upon hypothetical reasoning. Those discovered by M. Schmerling in the Liège caverns were similarly disposed of, and Dr. Lund's reports respecting partially petrified human bones, found by him in the interior of Brazil, in the same condition with those of numerous animals now extinct, which accompanied them, attracted no more than incredulous attention. In the caverns of Bizé, in France, human bones and shreds of pottery were found in red clay mixed with the *débris* of extinct mammalia; a similar collocation was soon after detected by M. de Serres, in the caverns of Pondres

* See Trans. Devon. Assoc., vol. iii., p. 334, 1869.

and Souvignargues; and Dr. Boué found human bones mixed with others of extinct species at Lahr. In 1833 human bones were found, together with several species of the well-known extinct cave mammals, in caves near Liège, beneath a thick coat of stalagmite; and about the same period, the Rev. Mr. MacEnery collected from the caves of Torquay, human bones and flint knives, amongst a great variety of extinct species, all under a crust of stalagmite, upon which the head of a wolf reposed. Amongst the bones of the mammoth and his contemporaries, found at Oreston, near Plymouth, at different times before and after that period, the upper portion of a humerus of man was detected, and immediately thrown away as valueless on being pointed out to the possessor. About the end of the last century, gypsum quarries were opened in the Vale of Kostritz, in Upper Saxony. The gypsum was intersected in every direction by caves and fissures, which were filled with red clay containing clusters of bones of mammalia, including man, elephant, rhinoceros, horse, ox, elk, deer, reindeer, a great felis, hyæna, hare, and rabbit. A fragment of an arm and a thigh-bone of a man were dug out of the clay at a depth of eighteen feet; and eight feet below two phalanges of a rhinoceros. The facts were carefully observed and first described by Baron von Sclotheim, who remarked of the human bones, that they were few, completely detached and isolated, always found with the other animal remains, under the same relations, not constituting connected skeletons, but gathered in various groups.*

In 1861, M. E. Lartet, the eminent French palæontologist, published a highly interesting description of a cave or grotto at Aurignac, in the south of France.

It appears that some years before the paper was written, a man breaking stones for road repairs, observed a rabbit run into a hole in a steep talus of *débris*, and was led to thrust his arm in after it. He was not a little surprised to find that he had seized and drawn out a large bone. Prompted by curiosity, he cut a passage through the talus until his progress was stayed by a large upright slab of stone, which, on being removed, was found to have closed the entrance of a small cavern, previously unknown and unsuspected, and in front of which it had certainly been artificially placed. In the grotto he found a quantity of human bones and skulls, which naturally attracted a

* *Op. cit.*, pp. 93—98.

large amount of attention in the neighbourhood, and were the theme of so much conjecture, that the mayor of Aurignac deemed it prudent that the human remains should be collected and re-interred in the parish burial ground. Fortunately the mayor was a medical man, and before his directions were carried out, he had ascertained that the bones represented seventeen human bodies.

In 1860, M. Lartet, passing through Aurignac, visited the cave, of which he had previously heard, and proceeded to a full personal investigation of the deposit, both within and immediately without the cavern. In the soil which the human bones had occupied, and in its immediate prolongation outside, he found the remains of from 80 to 100 individuals, belonging to nineteen species of mammals, some existing and some extinct, including the cave bear, cave lion, cave hyæna, *Elephas primigenius*, *Rhinoceros tichorhinus*, Irish elk, reindeer, and aurochs. With them were tools of flint and of deer's horn—chiefly reindeer, and an article made of a canine of a young cave bear, the use of which was unknown, but was perhaps interred with one of the bodies as a token of affection. The marrow-bones of the aurochs, reindeer, and rhinoceros had been split by man, and it was observed that some of those of the last named species which had been thus treated, had subsequently been gnawed by the hyænas, as had many others. Outside the grotto, under the talus of fallen *débris* but on the ossiferous earth, was a layer composed of ashes and charcoal, from six to eight inches thick, and covering an area of several square yards. It contained a very great many teeth, principally of herbivorous animals, together with many hundreds of fragments of their bones, and coprolites of the hyæna. By chemical examination it was found that the bones of the reindeer, aurochs, rhinoceros, &c., had retained precisely the same proportion of nitrogen as the human bones mixed with them.

The interpretation which the facts obviously suggested and required, was that the grotto was an ancient burial-place, closed ordinarily with the heavy slab found at the entrance, but opened from time to time for the introduction of another corpse; that symbolical and votive objects, as well, perhaps, as such as were supposed to be useful in another state of being, were interred with the bodies; that after each burial a feast was held immediately without the sepulchre; that the hyænas devoured the remnants of the feast after the departure of the relatives; and that the era

of the interments was that of the great cave bear and his contemporaries.*

The Committee appointed by the British Association in 1864 to explore Kent's Cavern, Torquay, reported in 1867, that in the stalagmitic floor overlying the cave earth, they had found a human tooth and part of an upper human jaw, containing four teeth; and, in 1868 as well as 1869, that in the same continuous sheet of stalagmite they had detected remains of the cave bear, cave hyæna, and rhinoceros. Moreover, that whilst the human remains were deeply imbedded in the stalagmite, where it was 20 inches thick, some of those of the extinct mammals were not only near its upper surface, but were partially exposed.†

The case of the famous human jaw of Moulin Quignon, near Abbeville, will probably be remembered by all our readers; and were we to pass it by in silence, we might be suspected of acquiescing in the opinion that it was an unquestionable forgery. The facts are very briefly as follow:—In March, 1863, a gravel-digger informed the late M. Boucher de Perthes, the distinguished Archæologist of Abbeville, that at 15 feet below the surface a bone was to be seen projecting about an inch from the face of a cutting then in progress, in a gravel pit at Moulin Quignon. M. de Perthes, accompanied by a friend, proceeded at once to the spot, where they witnessed the extraction of the bone, which proved to be an entire half of a human jaw, containing one tooth. The discovery at once attracted the attention it deserved; an Anglo-French scientific commission was appointed to investigate it, and after much experimentation and discussion, the opinion of a large majority that the jaw was found in an undisturbed portion of the lowest of five superimposed layers of gravel and clay, containing unpolished flint implements, and half a yard below fragments of the tooth of an extinct mammal, was embodied in a report drawn up by Professor Milne Edwards, and presented to the Academy of Sciences of Paris in May, 1863. The conclusion, however, was that of a majority only, for a few of the English Commissioners dissented; and this being the case, though by no means convinced that the minority was right, we doubt the wisdom of an unqualified acceptance of the authenticity of this famous jaw.‡

Such are the statements respecting the inosculation of the

* See Lyell's Antiquity of Man, pp. 181—193; or, Natural History Review, vol. ii., pp. 53—71.

† See Reports British Association, 1867, p. 28; 1868, p. 52; 1869, p. 204.

‡ See Anthropological Review, vol. i., pp. 166—168, 177—179, 312—335.

bones of man, and of the extinct cave mammals in undisturbed deposits, which, without special search, we have from time to time encountered and noted in the course of reading. Compared with the number of infra-human remains supposed to be contemporary which are piled up in numerous museums, the human relics are no doubt but few; yet we cannot but think that, without insisting on each individual case—though we know not why any should be excepted—they are sufficiently numerous and sufficiently well-attested to constitute a full and complete answer to the question, "Why don't you find the *bones* of the men as well as their implements?"

III. It may be questioned, perhaps, whether the acknowledged discovery of a portion of man's osseous system, in the very places occupied by the implements, would, or, indeed, ought to produce any change in the opinion of those who propose the sceptical interrogation. To doubt the human origin of the implements, of bone and stone, is utterly beyond the power of any one. They do or they do not prove that man was the contemporary of the animals with whose bones and teeth and horns they are found. If they do, his bones could do no more. If they do not, they must have found their way into the deposits since the relics of the extinct animals were lodged in them; and if this is possible in the case of human *tools*, why not also in the case of human *bones*?

But is the interrogation we have heard so often always proposed with perfect ingenuousness? Does it not, at least, sometimes remind us that "Drowning men catch at straws?" Do the proposers ever act similarly in the common affairs of life? Does any one ever think that De Foe's Crusoe was illogical, when he inferred that a man had been on his island simply because he saw a human foot-print on the sand? Or suppose that he should have waited until he had seen the *man* before he took the trouble to make himself secure?

Though by no means essential as evidence of the existence of man when the deposits in question were laid down, there can be no doubt that the exhumation of human bones would be a source of much pleasure; for if they did no more, they might teach us whether our early ancestors belonged to an extinct or to an existing branch of the human family, and what were their mental capabilities.

When we remember, however, that the implements which have been found imply that the intellectual position of their makers was low *actually*, whatever it may have been

potentially; that in a climate like ours a savage population must also be very sparse; that so far as we know savages either burn their dead or bury them, and that any not recovered by their friends would probably have been speedily disposed of by the hyænas and other beasts of prey which then abounded, we are by no means sanguine that many bones of the men of the period under consideration will ever be found, unless it be in an ancient cemetery, such as that at Aurignac, already mentioned; or that those who wish to retain it will speedily be deprived of the privilege of asking, "Why don't you find the *bones* of the men as well as their implements?"

V. EXPERIMENTAL INVESTIGATION OF A NEW FORCE.

By WILLIAM CROOKES, F.R.S., &c.

TWELVE months ago in this journal* I wrote an article, in which, after expressing in the most emphatic manner my belief in the occurrence, under certain circumstances, of phenomena inexplicable by any known natural laws, I indicated several tests which men of science had a right to demand before giving credence to the genuineness of these phenomena. Among the tests pointed out were, that a "delicately poised balance should be moved under test conditions;" and that some exhibition of power equivalent to so many "foot-pounds" should be "manifested in his laboratory, where the experimentalist could weigh, measure, and submit it to proper tests." I said, too, that I could not promise to enter fully into this subject, owing to the difficulties of obtaining opportunities, and the numerous failures attending the enquiry; moreover, that "the persons in whose presence these phenomena take place are few in number, and opportunities for experimenting with previously arranged apparatus are rarer still."

Opportunities having since offered for pursuing the investigation, I have gladly availed myself of them for applying to these phenomena careful scientific testing experiments, and I have thus arrived at certain definite results which I think it right should be published. These experiments appear conclusively to establish the existence of a new force, in some unknown manner connected with the human organisation, which for convenience may be called the Psychic Force.

* See Quarterly Journal of Science, vol. vii., p. 316, July, 1870.

Of all the persons endowed with a powerful development of this Psychic Force, and who have been termed "mediums" upon quite another theory of its origin, Mr. Daniel Dunglas Home is the most remarkable, and it is mainly owing to the many opportunities I have had of carrying on my investigation in his presence that I am enabled to affirm so conclusively the existence of this Force. The experiments I have tried have been very numerous, but owing to our imperfect knowledge of the conditions which favour or oppose the manifestations of this force, to the apparently capricious manner in which it is exerted, and to the fact that Mr. Home himself is subject to unaccountable ebbs and flows of the force, it has but seldom happened that a result obtained on one occasion could be subsequently confirmed and tested with apparatus specially contrived for the purpose.

Among the remarkable phenomena which occur under Mr. Home's influence, the most striking as well as the most easily tested with scientific accuracy are—(1) the alteration in the weight of bodies, and (2) the playing of tunes upon musical instruments (generally an accordion, for convenience of portability) without direct human intervention, under conditions rendering contact or connection with the keys impossible. Not until I had witnessed these facts some half-dozen times, and scrutinised them with all the critical acumen I possess, did I become convinced of their objective reality. Still, desiring to place the matter beyond the shadow of a doubt, I invited Mr. Home on several occasions to come to my own house, where in the presence of a few scientific enquirers, these phenomena could be submitted to crucial experiments.

The meetings took place in the evening, in a large room lighted by gas. The apparatus prepared for the purpose of testing the movements of the accordion, consisted of a cage, formed of two wooden hoops, respectively 1 foot 10 inches and 2 feet diameter, connected together by 12 narrow laths, each 1 foot 10 inches long, so as to form a drum-shaped frame, open at the top and bottom; round this 50 yards of insulated copper wire were wound in 24 rounds, each being rather less than an inch from its neighbour. These horizontal strands of wire were then netted together firmly with string, so as to form meshes rather less than 2 inches long by 1 inch high. The height of this cage was such that it would just slip under my dining table, but be too close to the top to allow of the hand being introduced into the interior, or to admit of a foot being pushed under-

neath it. In another room were two Grove's cells, wires being led from them into the dining-room for connection if desirable with the wire surrounding the cage.

The accordion was a new one, having been purchased for these experiments at Wheatstone's, in Conduit Street. Mr. Home had neither handled nor seen the instrument before the commencement of the test experiments.

In another part of the room an apparatus was fitted up for experimenting on the alteration in the weight of a body. It consisted of a mahogany board, 36 inches long by 9½ inches wide and 1 inch thick. At each end a strip of mahogany 1½ inches wide was screwed on, forming feet. One end of the board rested on a firm table, whilst the other end was supported by a spring balance hanging from a substantial tripod stand. The balance was fitted with a self-registering index, in such a manner that it would record the maximum weight indicated by the pointer. The apparatus was adjusted so that the mahogany board was horizontal, its foot resting flat on the support. In this position its weight was 3 lbs., as marked by the pointer of the balance.

Before Mr. Home entered the room the apparatus had been arranged in position, and he had not even had the object of some of it explained before sitting down. It may, perhaps, be worth while to add, for the purpose of anticipating some critical remarks which are likely to be made, that in the afternoon I called for Mr. Home at his apartments, and when there he suggested that as he had to change his dress, perhaps I should not object to continue our conversation in his bedroom. I am, therefore, enabled to state positively, that no machinery, apparatus, or contrivance of any sort was secreted about his person.

The investigators present on the test occasion were an eminent physicist, high in the ranks of the Royal Society, whom I will call Dr. A. B.; a well-known Serjeant-at-Law, whom I will call Serjeant C. D.; my brother; and my chemical assistant.*

Mr. Home sat in a low easy chair at the side of the table. Close in front under the table was the aforesaid cage, one of

* It argues ill for the boasted freedom of opinion among scientific men, that they have so long refused to institute a scientific investigation into the existence and nature of facts asserted by so many competent and credible witnesses, and which they are freely invited to examine when and where they please. For my own part I too much value the pursuit of truth, and the discovery of any new fact in nature, to avoid enquiry because it appears to clash with prevailing opinions. But as I have no right to assume that others are equally willing to do this, I refrain from mentioning the names of my friends without their permission.

his legs being on each side of it. I sat close to him on his left, and another observer sat close on his right, the rest of the party being seated at convenient distances round the table.

For the greater part of the evening, particularly when anything of importance was going forward, the observers on each side of Mr. Home kept their feet respectively on his feet, so as to be able to detect his least movement.

The temperature of the room varied from 68° to 70° F.

Mr. Home took the accordion between the thumb and middle finger of one hand at the opposite end to the keys (see woodcut, Fig. 1), (to save repetition this will be subsequently called "in the usual manner.") Having

Fig 1.

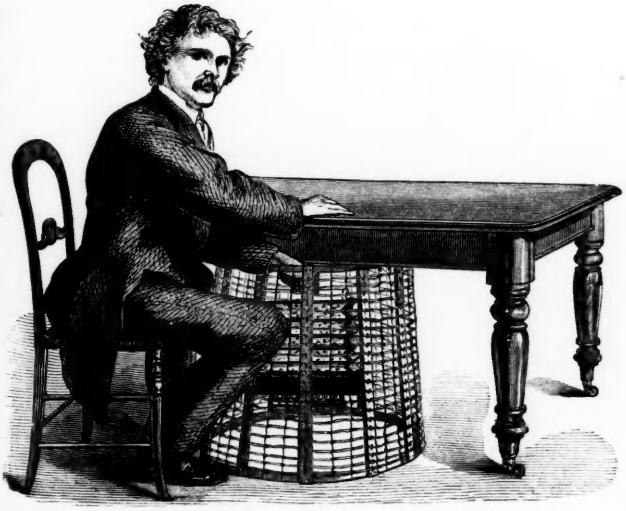
previously opened the bass key myself, and the cage being drawn from under the table so as just to allow the accordion to be passed in keys downwards, it was pushed back as close as Mr. Home's arm would permit, but without hiding his hand from those next to him (see Fig 2). Very soon the accordion was seen by those on each side to be waving about in a somewhat curious manner; then sounds came from it, and finally several notes were played in succession. Whilst this was going on, my assistant got under

the table, and reported that the accordion was expanding and contracting; at the same time it was seen that Mr. Home's hand which held it was quite still, his other hand resting on the table.

Presently the accordion was seen by those on either side of Mr. Home to move about, oscillating and going round and round the cage, and playing at the same time. Dr. A. B. now looked under the table, and said that Mr. Home's hand appeared quite still whilst the accordion was moving about emitting distinct sounds.

Mr. Home still holding the accordion in the usual manner in the cage, his feet being held by those next him,

FIG. 2.



and his other hand resting on the table, we heard distinct and separate notes sounded in succession, and then a simple air was played. As such a result could only have been produced by the various keys of the instrument being acted upon in harmonious succession, this was considered by those present to be a crucial experiment. But the sequel was still more striking, for Mr. Home then actually let go the accordion, removed his hand quite out of the cage, and placed it in the hand of the person next to him,

the instrument then continuing to play whilst no one was touching it.

I was now desirous of trying what would be the effect of passing the battery current round the insulated wire of the cage, and my assistant accordingly made the connection with the wires from the two Grove's cells. Mr. Home again held the instrument inside the cage in the same manner as before, when it immediately sounded and moved about vigorously. But whether the electric current passing round the cage assisted the manifestion of force inside, it is impossible to say.

The accordion was now again taken without any visible touch from Mr. Home's hand, which he removed from it entirely; I and two of the others present not only seeing his released hand, but the accordion also floating about with no visible support inside the cage. This was repeated a second time, after a short interval. Mr. Home presently re-inserted his hand in the cage and again took hold of the accordion. It then commenced to play, at first chords and runs, and afterwards a well-known sweet and plaintive melody, which it executed perfectly in a very beautiful manner. Whilst this tune was being played, I took hold of Mr. Home's arm, below the elbow, and gently slid my hand down it until I touched the top of the accordion. He was not moving a muscle. His other hand was on the table, visible to all, and his feet were under the feet of those next to him.

Having met with such striking results in the experiments with the accordion in the cage, we turned to the balance apparatus already described. Mr. Home placed the tips of his fingers lightly on the extreme end of the mahogany board which was resting on the support, whilst Dr. A. B. and myself sat, one on each side of it, watching for any effect which might be produced. Almost immediately the pointer of the balance was seen to descend. After a few seconds it rose again. This movement was repeated several times, as if by successive waves of the Psychic Force. The end of the board was observed to oscillate slowly up and down during the time.

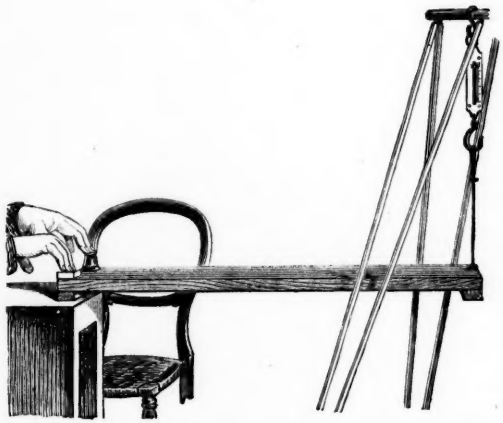
Mr. Home now of his own accord took a small hand-bell and a little card match-box, which happened to be near, and placed one under each hand, to satisfy us, as he said, that he was not producing the downward pressure (see Fig. 3). The very slow oscillation of the spring balance became more marked, and Dr. A. B., on watching the index, said that he saw it descend to 6½ lbs. The normal weight of the board

as so suspended being 3 lbs., the additional downward pull was therefore 3½ lbs. On looking immediately afterwards at the automatic register, we saw that the index had at one time descended as low as 9 lbs., showing a maximum pull of 6 lbs.

In order to see whether it was possible to produce much effect on the spring balance by pressure at the place where Mr. Home's fingers had been, I stepped upon the table and stood on one foot at the end of the board. Dr. A. B., who was observing the index of the balance, said that the whole weight of my body (140 lbs.) so applied only sunk the index 1½ lbs., or 2 lbs. when I jerked up and down. Mr. Home had been sitting in a low easy-chair, and could not, therefore, had he tried his utmost, have exerted any material influence on these results. I need scarcely add that his feet as well as his hands were closely watched by all in the room.

This experiment to me appears, if possible, more striking than the one with the accordion. As will be seen on referring to the cut (Fig. 3), the board was arranged perfectly horizontally, and it was particularly noticed that Mr. Home's fingers were not at any time advanced more than

Fig. 3.



1½ inches from the extreme end, as shown by a pencil-mark, which, with Dr. A. B.'s acquiescence, I made at the time. Now, the wooden foot being also 1½ inches wide, and resting flat on the table, it is evident that no amount of pressure exerted within this space of 1½ inches could produce any

action on the balance. Again, it is also evident that when the end furthest from Mr. Home sank, the board would turn on the further edge of this foot as on a fulcrum. The arrangement was consequently that of a see-saw, 36 inches in length, the fulcrum being 1½ inches from one end; were he therefore to have exerted a downward pressure, it would have been in opposition to the force which was causing the other end of the board to move down.

The slight downward pressure shown by the balance when I stood on the board was owing probably to my foot extending beyond this fulcrum.

I have now given a plain unvarnished statement of the facts from copious notes written at the time the occurrences were taking place, and copied out in full immediately after. Indeed, it would be fatal to the object I have in view—that of urging the scientific investigation of these phenomena—were I to exaggerate ever so little; for although to my readers Dr. A. B. is at present represented by incorporeal initials, to me the letters represent a power in the scientific world that would certainly convict me if I were to prove an untrustworthy narrator.

I confess I am surprised and pained at the timidity or apathy shown by scientific men in reference to this subject. Some little time ago, when an opportunity was first presented to me of examining into the subject, I invited the co-operation of some scientific friends in a systematic investigation; but I soon found out that to obtain a scientific committee for the investigation of this class of facts was out of the question, and that I must be content to rely on my own endeavours, aided by the co-operation from time to time of the few scientific and learned friends who were willing to join in the inquiry. I still feel that it would be better were such a committee of known men to be formed, who would meet Mr. Home in a fair and unbiassed manner, and I would gladly assist in its formation; but the difficulties in the way are great.

A committee of scientific men met Mr. Home some months ago at St. Petersburg. They had one meeting only, which was attended with negative results; and on the strength of this they published a report highly unfavourable to Mr. Home. The explanation of this failure, *which is all they have accused him of*, appears to me quite simple. Whatever the nature of Mr. Home's power, it is very variable, and at times entirely absent. It is obvious that the Russian experiment was tried when this force was at a minimum. The same thing has frequently happened within my own experience. A party of scientific men met Mr. Home

at my house, and the results were as negative as those at St. Petersburg. Instead, however, of throwing up the inquiry, we patiently repeated the trial a second and a third time, when we met with results which were positive.

These conclusions have not been arrived at hastily or on insufficient evidence. Although space will allow only the publication of the details of one trial, it must be clearly understood that for some time past I have been making similar experiments and with like results. The meeting on the occasion here described was for the purpose of confirming previous observations by the application of crucial tests, with carefully arranged apparatus, and in the presence of irreproachable witnesses.

Respecting the cause of these phenomena, the nature of the force to which, to avoid periphrasis, I have ventured to give the name of *Psychic*, and the correlation existing between that and the other forces of nature, it would be wrong to hazard the most vague hypothesis. Indeed, in enquiries connected so intimately with rare physiological and psychological conditions, it is the duty of the enquirer to abstain altogether from framing theories until he has accumulated a sufficient number of facts to form a substantial basis upon which to reason. In the presence of strange phenomena as yet unexplored and unexplained following each other in such rapid succession, I confess it is difficult to avoid clothing their record in language of a sensational character. But to be successful an enquiry of this kind must be undertaken by the philosopher without prejudice and without sentiment. Romantic and superstitious ideas should be entirely banished, and the steps of his investigation should be guided by intellect as cold and passionless as the instruments he uses. Having once satisfied himself that he is on the track of a new truth, that single object should animate him to pursue it, without regarding whether the facts which occur before his eyes are "naturally possible or impossible."

Since this article was in type, the Author has been favoured with the following letters from Dr. Huggins and Mr. Serjeant Cox—the Dr. A. B. and Serjeant C. D. therein referred to:—

Upper Tulse Hill, S.W.,
June 9, 1871.

DEAR MR. CROOKES,—Your proof appears to me to contain a correct statement of what took place in my presence at your house. My position at the table did not permit me to

be a witness to the withdrawal of Mr. Home's hand from the accordion, but such was stated to be the case at the time by yourself and by the person sitting on the other side of Mr. Home.

The experiments appear to me to show the importance of further investigation, but I wish it to be understood that I express no opinion as to the cause of the phenomena which took place. Yours very truly,

WILLIAM HUGGINS.

WM. CROOKES, Esq., F.R.S.

36, Russell Square,
June 8, 1871.

MY DEAR SIR,—Having been present, for the purpose of scrutiny, at the trial of the experiments reported in this paper, I readily bear my testimony to the perfect accuracy of your description of them, and to the care and caution with which the various crucial tests were applied.

The results appear to me conclusively to establish the important fact, that there is a force proceeding from the nerve-system capable of imparting motion and weight to solid bodies within the sphere of its influence.

I noticed that the force was exhibited in tremulous pulsations, and not in the form of steady continuous pressure, the indicator moving and falling incessantly throughout the experiment. This fact seems to me of great significance as tending to confirm the opinion that assigns its source to the nerve organisation, and it goes far to establish Dr. Richardson's important discovery of a nerve atmosphere of various intensity enveloping the human structure.

Your experiments completely confirm the conclusion at which the Investigation Committee of the Dialectical Society arrived, after more than forty meetings for trial and test.

Allow me to add that I can find no evidence even tending to prove that this force is other than a force proceeding from, or directly dependent upon, the human organisation, and therefore, like all other forces of nature, wholly within the province of that strictly scientific investigation to which you have been the first to subject it.

Psychology is a branch of science as yet almost entirely unexplored, and to the neglect of it is probably to be attributed the seemingly strange fact that the existence of this nerve-force should have so long remained untested, unexamined, and almost unrecognised.

XUM

Now that it is proved by mechanical tests to be a fact in nature (and if a fact, it is impossible to exaggerate its importance to physiology and the light it must throw upon the obscure laws of life, of mind, and the science of medicine) it cannot fail to command the immediate and most earnest examination and discussion by physiologists and by all who take an interest in that knowledge of "man," which has been truly termed "the noblest study of mankind." To avoid the appearance of any foregone conclusion, I would recommend the adoption of some appropriate name, and I venture to suggest that the force be termed the *Psychic Force;* the persons in whom it is manifested in extraordinary power *Psychics*; and the science relating to it *Psychism*, as being a branch of *Psychology*.

Permit me, also, to propose the early formation of a *Psychological Society*, purposely for the promotion of the study by means of experiment, papers, and discussion, of that hitherto neglected Science.—I am, &c.,

EDWD. WM. COX.

To W. CROOKES, Esq., F.R.S.

VI. MOLECULES, ULTIMATES, ATOMS, AND WAVES.

By MUNGO PONTON, F.R.S.E.

(Continued from p. 176).

PART II.

THE better to understand this subject it is well to have before the mind some definite conceptions respecting the luminiferous ether and its waves. The phenomena of heat, light, actinism, and fluorescence render it needful to assume the existence of an infinite ocean of ether, in which all ponderable bodies subsist. Each particle of this ethereal medium must be supposed to have a normal position in space from which it never departs, beyond the minute distance involved in a vibration. The ethereal particles must also be supposed to repel each other with an enormous energy, which, when they are disturbed, restores them in an inconceivably short space of time to their points of rest. Nevertheless, as this energy must act on every particle equally in all directions, any one or more of them may be moved from their points of rest by the application of a very slight extra-

neous impulse, so that the medium is perfectly elastic. Minute waves of various degrees of length are supposed to be propagated through this elastic ether, when any of its particles are disturbed. These waves manifest their existence in the phenomena of heat, light, actinism, and fluorescence —the longer waves in heat, the shorter in actinism and fluorescence, the intermediate in light.

According to the most recent determinations, the speed with which these waves traverse the ether is 185,000 miles in a second, or nearly 11,721-millionths of an inch in the billionth of a second—this last being the most convenient form in which to state the speed.

Great labour and skill have been bestowed on the measurement of the lengths of those minute waves. Fraünhofer, who first discovered the fixed lines which bear his name, measured the wave-lengths corresponding to the lines which he named B, C, D, E, F, and H, leaving behind him two sets of measurements, differing slightly from each other. More recent observations with more delicate appliances than were at his command, have shown the lines D, E, and H to be double. Professor Ångström of Upsal, availing himself of the best instrumental means that could be obtained, has re-measured the wave-lengths of all the principal lines from A to the more refracted H, inclusive. The results he has published along with an elaborate atlas of the spectrum, in which the lines are laid down according to their wave-lengths. The spectrum represented in this atlas is the diffracted or normal solar spectrum, obtained from a system of very fine equidistant lines. In all spectra produced by prisms the fixed lines are displaced relatively to each other, so that their true wave-lengths cannot be determined from any such spectra. This displacement is termed the irrationality of the spectrum—a phenomenon discussed at large in a paper inserted in the "Philosophical Magazine" for 1860, pp. 165, 263, and 364. This displacement is different in every different medium, and even in different sorts of glass; while it is more or less affected also by the temperature of the medium. Thus the fixed lines will have different positions relatively to each other in different spectroscopes and at different temperatures, while the greater the number of prisms the greater will be this source of error—a circumstance which has been rather overlooked in recent observations on spectrum analysis.

A plan of a portion of the solar spectrum has also been published by M. Kirchhoff, and of the remainder by Messrs. Hofmann, Ångström, and Thalén—all drawn to the same

scale—in which the lines are laid down as they appear to the eye when viewed through the spectroscope. But from the circumstance above explained, these positions are true only for the particular instruments employed in the observations. The positions given in this plan are also very diverse from those laid down in M. Ångström's atlas, which is constructed on a totally different principle, the lines being represented, not in the positions in which they appear to the eye, but in positions corresponding to their wave-lengths in ten-millionth parts of a millimetre. It would be desirable that a fresh plan were constructed combining both principles. The plan and the atlas both give the names of the vapours and gases by which the lines are supposed to be produced, in so far as these have been ascertained; but there is great difficulty in identifying any of the lines in the atlas with those on the plan, except in the case of the most conspicuous lines.

In 1859, when only the measures of Fraünhofer were known, it was shown in a paper inserted in the "Philosophical Magazine" for 1860, p. 437, that, assuming those measures to be approximately correct, it is probable that the wave-lengths of the principal lines are so connected by a law as to render their observed values a check one upon another. It has accordingly become interesting to ascertain how far this probability is confirmed by the more recent observations of M. Ångström. On investigation it is found that the measurements of the Swedish philosopher do follow such a law, but one differing from that which appeared deducible from those of Fraünhofer. A careful analysis of M. Ångström's measurements has proved the wave-lengths to be so mutually related that from the one corresponding to the more refrangible E, all the others may be accurately calculated by simple formulæ; while of the seven equations by which those values may be determined, the sum of the first three is equal to that of the remaining four, thus showing that all the wave-lengths are inter-dependent, each one upon the whole. These curious relations have been already enunciated in a memorandum added to a work recently published.* Craving reference to that memorandum for the formulæ by which they are expressed, it will suffice here to give the general result, by exhibiting in the following table the values of the wave-lengths, as calculated from those formulæ, and as given by M. Ångström from his observations,

* The Beginning; Its When and its How. Longmans and Co.

the unit being the ten-thousandth part of the millionth of a millimetre.*

	Observed.	Calculated.	Differences +.	Differences −.
A.	7,604,000	7,604,017	000,001,7	—
B.	6,867,100	6,867,098	—	000,000,2
C.	6,562,100	6,562,093	—	000,000,7
D.	5,895,130	5,895,117	—	000,001,3
E.	5,268,670	5,268,670	—	—
F.	4,860,740	4,860,747	000,000,7	—
G.	4,307,250	4,307,235	—	000,001,5
H.	3,933,000	3,932,995	—	000,000,5
			000,002,4	000,004,2

Thus the sum of the differences plus and minus is only 66-tenths of the thousandth of the millionth of a millimetre; while the greatest difference is only 17 of those decimal parts. This quantity would affect the position of the lines to an extent so minute as to be wholly inappreciable to the eye, either on the atlas of M. Ångström or on the map of M. Kirchhoff. The following are the calculated wave-lengths stated in billionths of an inch :—

A.	29,957,600	B.	27,054,360	C.	25,852,720
D.	23,225,300	E.	20,757,000	F.	19,149,930
G.	16,969,250	H.	15,494,850		

The extreme smallness of the differences between M. Ångström's measurements and the values calculated from the formulæ seems to establish both the accuracy of his observations and the truth of the relations which the formulæ express. The wave-lengths, as calculated from those formulæ, may accordingly be regarded as approximating very closely to the truth.

The relations thus shown to subsist among the wave-lengths, corresponding to the above eight principal fixed lines, will appear all the more remarkable when it is borne in mind that the waves themselves are due to the action of diverse substances. The A and B waves are produced by the action of atmospheric air; the C and F waves by hydrogen gas; the D wave by the vapour of sodium; the E, G, and H waves by the vapour of iron.

The marvel of the double coincidence, moreover,—the circumstance that not only are the other seven capable of being

* It is the longer of the two D waves, the shorter of the two E waves, and the shorter of the two H waves that are given in the table.

calculated from the wave-length of E alone, but that of the seven equations by which these are determined the sum of the first three is equal to that of the remaining four,—will be more fully appreciated by those who are able to estimate the vast amount of the probabilities against the existence of such a coincidence. It is also a curious, though accidental, coincidence that the wave-length of E expressed in ten-thousandths of the millionth of a millimetre is almost exactly equal to the tangent of an angle of $27^\circ\,47'$, namely, 5268685—the difference, 0000015, lying much within the limits of probable errors of observation, which are more than sixty times greater.* This tangent might accordingly be assumed as the value of E without affecting the relations of the wave-lengths to each other.

Having thus obtained the wave-lengths of the principal lines, it is easy to calculate the periods of time which the wave motion takes to traverse each wave-length in parts of the billionth of a second. For, seeing the motion is propagated at the rate of 11,721 millionths of an inch in the billionth of a second, we have only to divide this number by each wave-length, as given in the last table, to obtain the times. The following table exhibits the results in parts of the billionth of a second :—

A. One 391st.	E. One 565th.
B. One 433rd.	F. One 612th.
C. One 453rd.	G. One 691st.
D. One 505th.	H. One 756th.

The wave-length being the space traversed by the wave motion travelling onwards in a right line, while each ethereal particle embraced in it is performing a single vibration in a transverse direction, the above times are also the periods of vibration of the individual particles involved in each wave. It remains to ascertain what amount of motion the particles perform during the above minute fragments of time.

By comparing the speed of sound in traversing the air with that of light in passing through the ether, we learn that the elastic energy, operating in the latter case, exceeds in intensity that of terrestrial gravitation, operating in the former case 1,137,156 millions of times—the forces being to each other as the squares of the speeds. Terrestrial gravity being capable of dragging a particle through 193 inches in a

* The wave-lengths of F, when stated in hundred thousand millionths of an inch, makes a still nearer approach to being equal to the versed *sine* of an angle of $36^\circ\,3'\,1'' = 1914962$, which might be adopted as the value of the wave-length of F without affecting any of the calculations.

second, a force of the above number of times greater intensity would, in the same time, drag or drive it through nearly 219 billions of inches, or 219 billionths of an inch in the billionth of a second—the spaces being as the squares of the times.

The extreme distance to which a vibrating particle will depart from its point of rest will be that through which it may be dragged or driven by the returning force in the fourth part of the period of vibration. In the case of the wave A, it will be that through which it may be dragged or driven in the 1564th part of the billionth of a second. This gives the 11,169th part of the billionth of an inch for the departure of the particle from its point of rest. The length of the wave A being 29,956,800 billionths of an inch, this length is to the departure of each particle involved in that wave from its point of rest in the ratio of 334,600 millions to 1.

Seeing the spaces vary as the squares of the times, the ratio which the wave-lengths bear to the departure of the particles involved in them from their points of rest will be inversely proportional to the wave-lengths themselves. The shorter the wave the higher the ratio. The following table exhibits the ratio for the wave-lengths corresponding to the eight principal lines:—

A.	334,600 millions to	1.	E.	482,885 millions to	1.
B.	370,470 „	1.	F.	523,390 „	1.
C.	387,708 „	1.	G.	590,647 „	1.
D.	431,555 „	1.	H.	646,881 „	1.

A better notion of this immense excess of the length of the wave over the departure of the individual particles from their points of rest may be obtained by imagining both to be magnified a million of billions of times. The length of the A wave would then be nearly 473 millions of miles, while the departure of each particle involved in that wave from its point of rest would be only about $7\frac{1}{2}$ feet. In the case of the H wave, its length would be about 245 millions of miles, while the departure of each particle involved in that wave from its point of rest would be only about 2 feet.

This great excess of the wave-length over the departure from the point of rest shows the latter motion to be rateably very much slower than the speed with which the motion of translation runs along the wave-lengths from particle to particle—in other words, than the velocity of light. Thus in the case of the A wave the particles move the 11,169th part of the billionth of an inch in the 1564th part of the billionth of a second. Supposing the motion to be uniform, this rate is

only about a seventh of an inch in a second, during which period of time the wave motion travels 185,000 miles. The rate of motion of the particles is the same for all the waves, and the amazingly short period of time in which the vibration is accomplished results merely from the extreme minuteness of the motion performed.

The foregoing estimates apply to only that portion of the spectrum which is visible. But the thermal effects of the invisible ultra-red waves can be traced to a distance beyond A, which is equal to nearly five-eighths of the visible spectrum. As the wave-lengths increase by a geometrical progression, this distance would give for the longest wave-length whose existence can be ascertained nearly 4797 hundred millionths of an inch. Although this is only a rough approximation, it may for the present purpose be assumed as correct. Owing to the constitution of the ether, no particle can by a vibration be moved from its own point of rest so far as to become nearer to that of any other particle. There is thus a natural limit to the excursion of each individual particle—consequently to the length of the wave. Assuming the above to be the longest possible wave, the ratio which its length bears to the departure of the individual particles involved in it from their points of rest is 208,926 millions to 1; and about half this quantity, or, say, in round numbers, 100,000 millions to 1, will be the ratio which the longest wave-length will bear to the normal distance between the particles, the number of which in the longest wave will also be about 100,000 millions. This estimate may serve to convey some notion of the extreme smallness of the intervals between the particles of the luminiferous ether. What, then, must be the minuteness of the particles themselves?

From the nature of the wave motion and of the vibrations of the individual particles, the direction of which is across that in which the wave motion is propagated, it appears probable that, in any given line of propagation, or ray, the individual particles must depart wholly out of the line, so as to produce a complete interruption of its continuity, in order that the existence of the movement may be traceable by any physical effects;—in other words, the departure of the particles from their points of rest in all likelihood slightly exceeds their own radius. Could we, therefore, ascertain the probable length of the shortest wave whose existence can be traced by any physical effects, and the corresponding probable smallest departure of any particle from its point of rest, we might obtain a rough approximation to the probable

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diameter of the particles, which would be a little less than double that departure.

The late Professor W. A. Miller, of King's College, London, in an elaborate paper "On the Photographic Transparency of Various Bodies, and on the Photographic Effects of Metallic and other Spectra, obtained by means of the Electric Spark" ("Philosophical Transactions," 1862, p. 861), has shown how far the actinic effects of the spectrum, obtained from silver electrodes, can be traced beyond the line H. Dividing the portion of the spectrum between B and H into 16 equal parts, Professor Miller found that the greatest distance beyond H at which any photographic image could be produced with silver electrodes was 70·5 of these parts, or about 4·4 times the distance between B and H. This result could be obtained only when the light passed through a medium exerting little or no absorptive action on the most refrangible part of the spectrum, such as ice, water, white fluor spar, quartz, atmospheric air, hydrogen, carbonic oxide and carbonic acid gases. In all these media the actinic spectrum extended to the same limit beyond H. With zinc electrodes, however, an image was obtained at a point as far beyond H as 83 of the scale. This may be regarded as the limit of the actinic powers of the spectrum.

Professor Stokes, again, in a paper "On the Long Spectrum of Electric Light" ("Philosophical Transactions," 1862, p. 599) has shown that, with aluminium electrodes, a line can be traced a good way beyond this limit. What is remarkable, however, is that Professor Miller with aluminium electrodes could not obtain a photographic image at any point beyond 55 of his scale, much within the limit of the most refrangible zinc line. The very highly refrangible aluminium lines, discovered by Professor Stokes, render their existence manifest only by their effects on a strongly fluorescent salt of uranium, employed as a screen for their reception—quartz being used for the prism and lens. From a comparison of the diagrams given by Professor Stokes with those given by Professor Miller, it appears that the most highly refrangible wave whose existence is thus manifested by a bright line on the fluorescent screen is very nearly at 113 beyond H on Professor Miller's scale, or about 30 beyond the last zinc line.

No attempt appears to have been hitherto made to determine by actual measurement the wave-length corresponding to either the extreme zinc or the extreme aluminium line. Our only resource, therefore, is to make a rough estimate, founded on the supposition that the wave-lengths continue to follow a

geometrical progression. Thus estimated in hundredths of the millionths of an inch, the shortest actinic wave would be about 71, and the shortest wave capable of exciting fluorescence about 23·4—almost exactly 128 times, or 7 octaves shorter than the length of the wave A.*

If this be the correct length of the extreme aluminium waves, it will show that the fluorescence which it produces is analogous to a resonance in the case of sound, where a vibrating musical string establishes sympathetic vibrations in another string 7 octaves below it in the musical scale. The very short wave in the ether produced by the aluminium electrodes is not itself visible, but by acting on the molecules of the uranium salt, or on the chemical ultimates composing those molecules, or, perhaps, on the atoms constituting the ultimate of the metal uranium, they establish in them vibrations synchronous with themselves. These very rapid vibrations give rise by sympathy to others of a lower rate, an octave or more below their own—passing, perhaps, through several successive octaves until there become established in the fluorescent substance vibrations which are synchronous with some of those in the ethereal waves of the visible spectrum. There are thus produced in the ether return waves, which are of a length sufficient to affect the optic nerve.

On the supposition that the extreme aluminium wave is exactly 128 times shorter than the wave A, the proportion which the length of the wave bears to the departure from their points of rest of the individual particles involved in it will be 128 times greater than it is in the case of the A wave. This would make the proportion nearly 43 billions to 1, while the probable number of ethereal particles included in the length of this wave would be about 488 millions.

To convey a more precise idea, let us again apply a magnifying power of a million of billion times. The length of the shortest wave would then be about 3,702,300 miles, while the departure of each particle from its point of rest would be only 0·00547 decimal parts of an inch.

We are now in a position to form a pretty accurate

* Might not these invisible ultra-violet waves be turned to some useful purpose? Might not the aluminium waves be used for secret telegraphy by night, these invisible rays alone being sent forth in parallel lines by means of a parabolic reflector, while at the receiving station they might be concentrated by another parabolic reflector into a focus on a highly fluorescent surface, by which they would be rendered visible? Might not also the invisible zinc rays be employed to photograph an object secretly in the dark, by directing them on the object, and receiving them into the camera through a quartz lens?

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conception of a luminous wave as it exists in the free ether, if attention be confined to a single line of ethereal particles lying in the direction in which the wave motion is travelling. Take for example the wave F, one of those produced by hydrogen gas. Its length in hundredths of the millionth of an inch is about 1915, the probable number of ethereal particles embraced in this length is about 40,000 millions, all of which are more or less moving simultaneously in a direction transverse to that in which the wave is advancing. The foremost—that at the point of the wave—is just beginning to move. At a quarter of the length of the wave from this point, there is a particle which has performed the first quarter of its motion, and has departed to the farthest limit from its point of rest on one side. It has been in motion one 2448th part of the billionth of a second, and in that interval has travelled one 523,390th part of the millionth of the length of the wave, moving at the average rate of about one-seventh of an inch in a second. At the middle point of the wave is a particle which is passing through the line of propagation on its way to the other side of that line. It has been double of the above period of time in motion, and has travelled double the space. At three quarters of the wave-length from the front is a particle which has performed three-fourths of its movement. It has gone as far to the other side of the line of propagation as the limit of departure from its point of rest, having been in motion one 918th part of the billionth of a second. At the extreme rear of the wave is a particle which has just returned to its point of rest, having entirely completed its excursion in one 612th part of the billionth of a second. The particles intervening between each pair of the five above specified are, according to their position, in intermediate phases of their movement.

The perfect elasticity of the ethereal fluid leads to the inference that the rearmost particle, having regained its point of rest, will remain there until disturbed by a fresh impulse travelling along the same line of propagation. It will have delivered over to those in front of it the whole of the motive energy which it had acquired from the primary impulse. Owing, moreover, to the circumstance that all ponderable particles are in a continual state of rapid motion through the ether, it is improbable that any individual line of ethereal particles will be agitated twice in succession by one and the same ponderable particle, although it may be agitated by one of the same kind, and thus have transmitted along it a succession of

waves of the same length. The waves, however, travelling along any single line of propagation may be separated from each other by long intervals. The greater or less rapidity with which they succeed one another, will merely give rise to a greater or less intensity of effect at the recipient surface; a greater or less brightness in the case of the luminous waves; a greater or less heating effect in the case of the thermal waves; a greater or less photographic effect in the case of the actinic waves; a greater or less amount of fluorescence in the case of those waves which stimulate that property into action. In the case of luminous waves, they might be separated by intervals of 18,500 miles, and yet produce continuous vision; but waves separated by intervals of only a mile would produce a very great amount of intensity. It is owing to this largeness of the interval between the waves travelling along any individual line of propagation, that rays from different sources are found to cross each other in all directions without interfering one with another.

(To be continued).

VII. A NEW MECHANICAL AGENT.

A JET OF SAND.

HOW to cut or carve, mechanically, hard substances, such as stone, glass, or hard metals, in an expeditious, accurate, and economical manner, has always engaged the attention of engineers. At the present time the rapidly increasing cost of manual labour makes improvements in this direction more needful. The discovery and utilisation of opaque crystallised carbon, cheaper than transparent diamonds, but perhaps equally durable, has gone far in this direction. Now, Mr. B. C. Tilghman, of Philadelphia, comes forward, and shows that a jet of quartz sand thrown against a block of solid corundum will bore a hole through it 1½ inches in diameter, 1½ inches deep, in 25 minutes, and this with a velocity obtainable by the use of steam as a propelling power, at a pressure of 300 pounds per square inch—a remarkable result, when we consider that corundum is next to and but little inferior to the diamond in hardness.

At the meeting of the Franklin Institute, held February 15th, 1871, the resident secretary, Dr. W. H. Wahl, introduced this invention, illustrating his description of it by

practically cutting or depolishing the surface of a plate of glass by a sand blast of very moderate intensity. Various examples of hard substances cut, depolished, and carved into shape, were displayed. In the discussion which followed the presentation of this very remarkable discovery, Mr. Robert Briggs, in his interesting remarks on the subject, took occasion to say that it had been long remarked that window glass, exposed to the wind-driven sand, near the sea-shore, soon loses its polish, and cited some other well-known examples of the erosion of surface when exposed to a continued stream of moving particles. When we think of the many such examples, and consider that engineers have had continually to make provision against this well-known cutting effect, it seems surprising that it should not have been turned to some good account before this.

The following description of the process is taken from a paper by Mr. Coleman Sellers communicated to the "Journal of the Franklin Institute," advance sheets of which we owe to the courtesy of the editor, Dr. W. H. Wahl.

Mr. Tilghman's attention seems first to have been directed towards cutting stone or hard metal by a jet of sand impelled by steam escaping under high pressure. His early experiments were with very high pressure, but as he progressed in the knowledge of the results obtainable with various velocities, a great use for this process seemed to develope itself in sand driven by moderate air blasts, and applied to grinding or depolishing glass for ornamental purposes.

For grinding glass he uses a common rotary fan, 30 inches in diameter, making about 1500 revolutions per minute, which gives a blast of air of the pressure of about 4 inches of water, through a vertical tube, 2 feet high by 60 inches long, and 1 inch wide.

Into the top of this tube the sand is fed, and falling into the air current and acquiring velocity from it, is dashed down against the sheets of glass, which are slowly moved across, about 1 inch below the end of the tube. About 10 or 15 seconds' exposure to the sand blast is sufficient to completely grind or depolish the surface of ordinary glass; so that sheets of it carried on endless belts may be passed under this 1-inch wide sand shower at the rate of 5 inches forward movement per minute. In the machine in use for this purpose the spent sand is re-conveyed to the upper hopper by elevators, and the dust made by the sand blast (which might otherwise be a source of annoyance to the workman) is drawn back into the fan, and thence passes with the wind into the

blast tun, and again mingles with the shower of sand upon the glass.

By covering parts of the glass surface by a stencil or pattern of any tough or elastic material, such as paper, lace, caoutchouc, or oil paint, designs of any kind may be engraved.

There is a kind of coloured glass made by having a thin stratum of coloured glass melted or "flashed" on one side of an ordinary sheet of clear glass. If a stencil of sufficient toughness is placed on the coloured side, and exposed to the sand blast, the pattern can be cut through the coloured stratum in from about 4 to 20 minutes, according to its thickness.

The theoretical velocity of a current of air of the pressure of four inches of water, he calculates, is (neglecting friction) about 135 feet per second; the actual velocity of the sand is doubtless much less.

If a current of air of less velocity is used, say about 1 inch of water, very delicate materials, such as the green leaves of the fern, will resist a stream of fine sand long enough to allow their outlines to be engraved on glass. By graduating the time of exposure with sufficient nicety, so as to allow the thin parts of the leaves to be partly cut through by the sand, while the thicker central ribs and their branches still resist, the effect of a shaded engraving may be produced.

The grinding of such a hard substance as glass by an agent which is resisted by such a fragile material as a green leaf, seems at first rather singular. The probable explanation is, that each grain of sand which strikes with its sharp angle on the glass pulverises an infinitesimal portion, which is blown away as dust, while the grains which strike the leaf rebound from its soft elastic surface.

The film of bichromatised gelatine used as a photographic negative may be sufficiently thick to allow a picture to be engraved on glass by fine sand, driven by a gentle blast of air.

For cutting stone the inventor uses steam as the impelling jet; the higher the pressure, the greater is the velocity imparted to the sand, and the more rapid its cutting effect.

In using steam of about 100 pounds pressure, the sand is introduced by a central iron tube, about 3-16th inch bore, while the steam is made to issue from an annular passage surrounding the sand tube.

A certain amount of suction of air is thus produced, which draws the sand through the sand tube into the steam jet, and both are then driven together through a tube about 6 inches long, in which the steam imparts its velocity to

the sand, and finally strikes on the stone, which is held about an inch distant from the end of tube.

At the spot struck a red light is visible, as if the stone was red-hot, though really it is below 212° F. The light is probably caused by the breaking up of the crystals of the sand and stone.

The cutting effect is greatest when free escape is allowed for the spent sand and steam. In making a hole of a diameter but slightly greater than that of the steam jet, the rebounding steam and sand greatly interfere with and lessen the efficiency of the jet.

Under favourable conditions, using steam which was estimated as equal to about 1½ horse-power, at a pressure of about 125 pounds, the cutting effect per minute was about 1½ cubic inches of granite, or 3 cubic inches of marble, or 10 cubic inches of soft brown sandstone.

By means of flexible or jointed connecting tubes, the blast-pipe is made movable in any direction: grooves and mouldings of almost any shape can thus be made; or by means of stencil plates, letters or ornaments can be cut either in relief or intaglio with great rapidity in the hardest stone.

At a high velocity, quartz sand will cut substances much harder than itself, as before stated. With a steam jet of 300 pounds pressure, a hole 1½ inches in diameter was cut through a piece of corundum, 1½ inches thick, in 25 minutes.

A hole 1 inch long and ¼ inch wide was cut through a hard steel file ¼ inch thick, in 10 minutes, with a jet of 100 pounds steam.

A stream of small lead shot, driven by 50 pounds steam, wore a small hole in a piece of hard quartz; the shot were found to be only very slightly flattened by the blow, showing their velocity to have been moderate.

Among the curious examples of glass cut by this sand blast was shown a piece of ordinary window-glass, which, having been partially protected by a covering of wire gauze, had been cut entirely through, thus producing a glass sieve, with openings of about 1-12th of an inch, the intervening glass meshes being only 1-16th of an inch wide. This seems to have been produced more as a curiosity than for any practical purpose. Should such a sheet of perforated glass be required, it is questionable if it could be produced from a solid sheet by any other method.

A microscopic examination of the sheet glass depolished by this process shows a succession of pits formed by the

blows of the impinging grains of sand, and looks more uniform than do surfaces ground by any rubbing process.

This steam sand jet has already been introduced to clean cast-iron hollow ware previous to tinning the interior. Heretofore the interior surface has been turned, it having been found necessary to remove a thin shaving in a lathe to obtain a clean surface. The surface is cleaned more rapidly by the sand blast, and even more perfectly, because it penetrates into any holes or depressions which the turning tool could not reach. It is also probable that the sand striking the particles of plumbago, which separate the particles of metallic iron in ordinary grey cast-iron, will remove them, and thus expose a continuous metallic surface to take the tin.

In this relation Mr. Sellers notes that about twenty-five years ago, some experiments were made in Cincinnati, at the establishment of Mr. Miles Greenwood, by his brother, Mr. George Escol Sellers, with a view to making tinned hollow ware of ordinary grey iron. He made a machine for scouring the inside of the pots and kettles with sand and water; afterwards the still wet scoured surfaces passed into the chloride of zinc solution, and thence into the molten metal, and were uniformly tinned. For some reason, the process was not continued, and now it is only recorded as an abandoned invention, never before made public. The wet sand grinding could not, in this case, have been so efficient as Mr. Tilghman's sand blast. To speculate on the various uses to which this process may be applied, would not serve any good end, and would take up too much space. With this discovery we can hardly help recurring to the works of the ancients, and wondering if some such process could have aided the workers in the stone age, or could have been used in carving the Egyptian hieroglyphics. It has been noted by those familiar with the cutting or dressing of stone, that some materials, such as granite, are very much injured, or "stunned," by the blows of the cutting tool, and after being hand dressed a thickness of perhaps from $\frac{1}{8}$th inch to $\frac{1}{4}$th inch has to be ground away, to produce a solid uniform surface. By this sand cutting process the surface is not injured, is not "stunned," and is ready for polishing at once.

One curious fact connected with its use is, that when a surface to be cut in intaglio or otherwise is partially protected by templates of metal, these templates curl up under the blows of the sand, so that paper patterns are really more durable than patterns cut from brass. Sheet

steel, cut into shape and then hardened, will also curl up under the blows of the fine particles of sand, unless protected by sheets of yielding material. Fine lace will protect glass during the depolishing process, and leave its designs in polished lines on a ground surface.

At a recent meeting of the Photographic Society of Philadelphia, Mr. Tilghman submitted some specimens of engraving on glass, prepared by this beautiful sand process. A bichromatised gelatine negative is taken on glass from an engraving. This is then subjected to a stream of sand under a pressure of 1 to 4 inches of water. The gelatine film protects the glass, the parts not covered by it being cut by the sand. The process is complete in from three to ten minutes. The finest specimens are produced by using fine sifted sand at about 1 inch pressure, and a longer time of exposure.

VIII. BRITISH SERVICE ORDNANCE, 1870, 1871.

By S. P. Oliver, Lieut. Royal Artillery.

THE sixth annual "List of Service Ordnance and Ammunition" for 1871 has just been published by the Royal Artillery Institution, and by comparing it with the list of June, 1870, we are able to note the progress and change in the ever steadily advancing history of British gunnery.

The following abstract of changes effected during the last twelve months therefore may be worth noting.

To the superficial observer, indeed, the two lists mentioned above would appear almost identical, but the initiated soon detects important changes.

To commence with the rifled guns. In June, 1870, we had, and still retain in our service, two natures of 7-inch breech-loading screw polygrooved guns (the old Armstrong 100-pounder), two 40-pounders, three 20-pounders, one 9-pounder, and one 6-pounder, all Armstrong guns; the only other breech-loading guns in the service were, and now are, the 64-pounder and 40-pounder wedge guns. As regards all descriptions of breech-loading guns there has been no change, and this section of the list for 1870 remains unaltered in that for 1871.

It is in the section relating to muzzle-loading rifled guns alone that any change is to be noted. Of this description there are two bronze guns, the 9-pounder and the 7-pounder,

the latter grooved on the French principle, and the former on the same modified. The first gun was intended for Indian service only, and so marked in No. 5 list, but the note to this effect has been removed in this year's list. We now come to the first change, viz., in the cast-iron guns with an inner barrel of coiled wrought-iron. In No. 5 list there was only one in the service, viz., a 64-pounder of 71 cwt. (the old 8-inch smooth-bore altered). This year's list gives two more 64-pounders as added to our service ordnance, viz., an altered 68-pounder for Indian service, and a bored-up 32-pounder, both land service guns; all the 64-pounders are plain grooved, and also take the 32-pounder smooth bore ammunition.

The great change of the year, however, is undoubtedly the next item on the list; not as regards diameter of bore, but as regards weight and strength, an immense stride has been made. The list of wrought-iron guns last year was headed by the 12-inch gun of 25 tons weight, and perhaps more popularly known as the 600-pounder. The 12-inch is now preceded by the apparently reduced diameter of 11·6 inch, but under the heading of nominal weight and in the column which represents tons appear the ominous figures 35. These figures may well cause dismay to those gunners who, knowing the difficulty they have experienced in mounting 25-ton or even 12-ton guns, look forward to the questionable pleasure of having to place a "*Woolwich Infant*" in position; and considering that whole pamphlets have been written on the mere mounting of 12-ton guns at Malta,* we may expect large volumes on the handling of this infant monstrosity. Fortunately only one appeared at birth, although it is possible that Mr. Cardwell ardently desired twins, but the Woolwich factory is again in travail, and we may shortly expect an addition or two to the wrought-iron family. This latest gun, the gun of the period, is at present intended for sea service only, and will not improbably be placed on board the *Devastation* now building at Portsmouth.

After the 12-inch gun another 25-ton gun; the 11-inch is an addition to the list; the 10-inch, 9-inch, 8-inch, and two 7-inch guns remain in *statu quo*. All the above wrought-iron guns are rifled on the Woolwich system. To the three sea service 64-pounder shunt guns which succeed next in order, there is added this year a fourth plain grooved for land service. The remaining additions to last year's list of

* Proceedings of the Royal Artillery Institution.

wrought-iron guns consist of a 40-pounder, a 25-pounder, a 16-pounder, and two 9-pounders; the three first of which are not, indeed, yet *officially* introduced into the service, but are practically so. The steel 7-pounder gun completes the tale of the rifled ordnance at present in our service.

A short summary of the well known old smooth bore ordnance still in use will complete our brief survey of our modern British cannon.

Of bronze we have the following natures, viz., 12-pounder, 9-pounder, 6-pounder, and two 3-pounder guns; together with 32-pounder, 24-pounder, 12-pounder, and 4·4 inch howitzers; also two bronze mortars, the *Royal* and *Cœhorn*.

The after-mentioned pieces are all cast-iron. Of these there are still four carronades not yet obsolete, the 68-pounder, 32-pounder, 24-pounder, and 12-pounder. Of guns, we have two 68-pounders, of 112 and 95 cwt. respectively, one 10-inch Dundas shell gun, and three 8-inch (two of Millar's and one of Dundas's construction); the 56-pounder and the two patterns of 42-pounders are only retained in the service until replaced by rifled guns.

Next in order come the 32-pounder guns, of which there are more varied patterns than of any other gun in the service, there being no fewer than eleven descriptions, varying in weight from 63 cwt. to 25 cwt.; comprising one Millar, two Dundas, three Blomefield's (two of which are bored-up 24-pounders), three of Monk's (A, B, and C), one Dickson, and one Congreve; certainly a very miscellaneous but useful array of weapons: their diversity of size and pattern originally arose from the varied classes of vessels in the navy for which they were introduced or adapted from time to time.

Three 24-pounders, and four 18-pounders, two 12-pounders, and three 9-pounders, finish the list of cast-iron guns. The three howitzers of 10-inch, 8-inch, and 5½-inch, need no comment beyond noticing that the last will be replaced by a rifled gun. There now remain only the mortars to be disposed of; and here we may remark that Mr. Mallet is confident of producing a 36-inch mortar weighing 35 tons, which will meet the requirements and tests which his last mortars, built under the auspices of Lord Palmerston, failed to stand. Of our present mortars the 13-inch sea service is by far the most formidable; it is a grand weapon. Besides it we have two other 13-inch mortars. Those who wish to see what a heavy mortar battery is like should visit the fine battery at Puckpool, not far from Ryde, in the Isle of Wight. This apparently (from the sea) innocuous looking earthwork, hides

one of the most terribly destructive batteries in the world. After the 13-inch mortars we have two 10-inch and one 8-inch mortar. Such, according to the authority we have quoted, is the "*story of the guns*" at present, and we venture to hope that no other military state could show a list to surpass our actual supply of guns. The battle of Dorking will not be fought this year anyhow.

To conclude our notice of the sheet in question, we may mention another innovation in the right direction; this is a short table giving the muzzle velocities calculated from the latest experiments with our rifled breech and muzzle-loading ordnance, from which we may quote one illustrative example. For instance, we learn that the 600-pounder Palliser large capacity shell leaves the muzzle of the 12-inch 25-ton gun with a velocity of 1168 feet per second, when the full battering charge of 67 lbs. of the "*brutal*" R.L.G. powder is fired; on the other hand, when the milder pebble powder is used, a charge of 85 lbs. can be fired, giving the same projectile a muzzle velocity of 1300 feet per second. The greatest muzzle velocity given is that of the 7-inch gun of 6½ tons, the 115 lbs. shell of which is projected by 30 lbs. of pebble powder, with the tremendous velocity of 1525 feet per second.

It may also be remarked that this year's list contains besides the "Marks on cast-iron ordnance referring to their vents," &c., some additional useful memoranda as to the "Chief points to be remembered in considering whether a given fuze will suit any shell." In these days, when varieties of shell and fuzes are multiplied daily, and when many varied descriptions of each are found in a single battery, these last instructions should be conspicuously placed in every shell-room.

NOTICES OF BOOKS.

A Memoir on the Indian Surveys. By CLEMENTS B. MARKHAM. Printed by order of Her Majesty's Secretary of State for India in Council. W. H. Allen and Co., 13, Waterloo Place. London, 1871.

THE object of this work is to furnish a general view of all the surveying and other geographical operations in India from their commencement. It is divided into seventeen sections, comprising the Indian Marine Surveys; route surveys; the trigonometrical, topographical, geological, and archæological surveys; meteorological and tidal observations; astronomical observations; and the physical geography of India. It would be impossible to give a comprehensive review of a volume embodying so much information, hitherto contained only in official records, or scattered amongst the pages of almost innumerable publications, within the scope of which we are limited; a few remarks only to show the nature of the book before us must therefore suffice. The records from which information has been gleaned date back over 250 years. "The Surveys of India began along the coasts, and the sailors preceded the shore-going surveyors by nearly 200 years. Before India could be measured it was necessary to get there; and the history of Indian Surveying takes us back to the day when James Lancaster's fleet of four ships and a victualler got under way from Torbay, on the 2nd of May, 1601." Richard Hakluyt, it appears, was appointed historiographer of the East Indies in the same year; to him was entrusted the custody of all the journals of the East India voyages, and he gave lectures relating to them to the students at Oxford. The records of early voyages and of the surveys made by the Bombay Marine reached from 1601 to 1830. These latter were continued by the Indian Navy from 1832 to 1862, when it was arranged that all further surveys that might be required should be conducted by the Royal Navy.

The land survey and mapping of British India have advanced with the acquisitions of territory; they were commenced when the first battles were fought and the first provinces gained. Rennell, the father of Indian geography, served under Clive, the conqueror of Plassy. At that time all existing knowledge of India, derived from routes of solitary travellers and rough charts of the coasts, had just been collected and utilised by the great French geographer, D'Anville. His map of India appeared five years before the date of the battle of Plassy; and eight years afterwards Rennell was at work in the newly acquired territory of Bengal and Behar, laying the foundation for the construction of a map which was destined to succeed the admirable work of D'Anville. Brief accounts of the Trigonometrical, Topographical, Revenue,

and Geological Surveys of India appeared in the number of this Journal for October, 1870, and we shall not, therefore, further refer to those subjects beyond stating that they are given at much greater length and completeness in the volume now before us.

The Archæological Survey of India was for a long time carried on by independent antiquarians, who published their labours in the journals of different Indian societies, and they appear to have acted without any system and but little communication with one another. General Cunningham published his views on archæological investigation in 1848, but it was not until 1860 that the Government of India instituted an archæological survey, with the object of preserving ancient monuments, rendering them easy of access, obtaining correct copies of inscriptions and pieces of sculpture, and thus facilitating the studies of future antiquarians and historians.

Meteorological observations appear to have been taken in India so far back as the year 1785; and the earliest recorded tidal observations were taken at the Kidderpore Dock-head, on the Hooghly, by Mr. James Kyd, in 1806. Astronomical observations were commenced by the Government at Madras, in 1787, but the results of similar observations made by native astronomers so long ago as the 5th century, are preserved to the present day.

The physical geography of India is to a great extent written in the histories of travellers, and Mr. Markham has extracted from the works of numerous authors some very interesting information on this subject. Indeed, throughout the entire volume dry facts are interspersed with interesting historical information; and although its title would not probably commend itself to any but scientific readers, there is nevertheless much in it which will well repay others for the trouble of reading it, whilst the style in which it is written is easy and fluent.

Classical and Prehistoric Influences upon British History: our Philanthropy from of Old, our ever struggling Past, and our Future. By SAXE BANNISTER, M.A., formerly Attorney-General of New South Wales. Second Edition. London: Longmans, 1871.

MR. BANNISTER has a theory to uphold, and a very good theory it is, viz., that our peaceableness, our consideration for our colonies, and our elevation of the native nations which impinge upon our possessions rather than a desire to dispossess or exterminate them, as is openly proclaimed by one class of politicians as a decree of inevitable Fate, will tend to the prolonged existence of the nation as a ruling power, whereas the opposite course will bring upon us a destruction as complete and probably as speedy as that of the Roman Empire, produced as it was by the wanton conquests and tyrannical oppression of

conquered nations. This theory plainly stated and supported by the simple arguments that naturally arise, would no doubt find a response in the mind of every thoughtful man whose passions were not inflamed by preconceived notions or interested motives. But we hardly think that the pamphlet of Mr. Bannister, in which he endeavours to show that this theory was the ruling principle of the ancient Britons or other inhabitants of this land before the Roman conquests, and that the reverse was the guiding motive of the Romans during the whole period of the empire, a motive which led directly to their downfall, will greatly advance the end he has in view. The references to early writers, without quoting their actual words and without discussing the weight of authority that each deserves, will scarcely influence the critical minds of the present day, and it can only be to minds thoroughly imbued with an historical tone that such an argument can be addressed. We would rather meet with Mr. Bannister as an advocate than as an historian.

An Introductory Text-Book of Zoology, for the use of Junior Classes. By H. Alleyne Nicholson, M.D., D.Sc., &c., &c., Lecturer on Natural History in the Medical School of Edinburgh, &c. Wm. Blackwood and Sons, Edinburgh and London, 1871.

This work is just what it professes to be. In plain and simple language it describes the characteristic differences between the various sub-kingdoms, classes, and orders of the animal kingdom. The technical terms introduced are not only explained as they occur, but their derivations are given, and a glossary at the end enables the student to refer again to the meaning of the word should it occur a second time. The illustrations are numerous, and are chosen, where possible, from subjects of common occurrence, and include both external appearance and internal structure. The book is well suited to become the text-book for schools, and contains nothing that an ordinary schoolboy of 13 or 14 could not understand. We hail such a work as tending to the introduction of Natural History teaching in schools, a subject which has hitherto been neglected owing greatly to the want of such text-books as the one before us.

Romance of Motion, or a Mode of Motion of the Planetary Bodies in Space; also a Hypothetical Analysis and Synthesis of Nitrogen. By Alec Lee. London: Longmans, Green, and Co., 1871.

This work is the result of Imagination applied to the Physical Sciences. Starting from the hypothesis that the ether, which

pervades space as demonstrated to us by the passage of light and heat, must be resistant to the motions of the heavenly bodies, Mr. Lee would find a counteractant force in the magnetism of the sun acting on the planets as diamagnetic bodies, and he then suggests that as magne-crystals they would in certain positions retard their velocities, thus causing the varieties of speed at various portions of their orbits. Neither for this nor for his theory that nitrogen is a compound of C_3H_4O does he give the slightest proof beyond the mere fitness of things. We must, therefore, decline to consider either proved, although the atomic weight of the elements divided by the reduced volume of the gas after combination does give the specific gravity of nitrogen.

The Moabite Stone, a fac simile of the Original Inscription, with an English Translation, and a Historical and Critical Commentary. By CHRISTIAN D. GINSBURY, LL.D. London: Longmans, 1870.

THIS monograph not only contains, as its title-page describes, the original inscription in *fac simile*, a translation, and a commentary, but it also gives a transliteration of the inscription in modern Hebrew characters, so that any one but moderately instructed in that language can follow the text; and it gives also a history of the discovery and destruction of the stone itself, together with six translations besides that of the editor, viz., those of Professors Noeldeke, Haug, and Schlottman, and those of MM. Ganneau, Neubauer, and Derenbourg.

With the historical and theological value of this monument, believed to be the most ancient extant inscription in this branch of the Semitic languages, we have here nothing to do; but we may call attention to the extreme value of the inscription from an alphabetic and linguistic point of view. Though it has been long known from internal evidence as well as from tradition that the main portion of the Greek Alphabet was derived from the Phœnician, *i.e.*, the old Hebrew; tradition, which had been right in the main outline, declared that some few of the letters were the addition of the Greeks themselves. This the inscription proves to be untrue, for not only are the earliest forms of these doubtful letters found in the Moabite alphabet, but they correspond in position to the letters supposed to be peculiarly Greek. Thus we can complete the pedigree of all our letters from the present day back to the Syrian races, at least as early as the 10th century, B.C.

Linguistically, also, the stone is valuable. It shows that the language of Moab was less distinguished from that of Judea than was that of Phœnicia—a very moderate Hebrew scholar could read the inscription in many parts without difficulty. It shows also that the traditional vowel points, which are known to

have been inserted at a late period, have some amount of authority, and that the *matres lectionis*, which some have supposed to be late additions to the Hebrew text, were then, as now, sometimes used and sometimes omitted.

The Year Book of Facts in Science and Art, &c.. By JOHN TIMBS. London: Lockwood and Co., 1871.

THE Year Book of Facts is a compilation consisting almost exclusively of cuttings from various newspapers, from periodicals—more or less scientific—and from the proceedings of learned societies. A portrait and life of Professor Huxley begin the book, and these are accompanied by an abstract of the President's address to the British Association at Liverpool, and some account of his other works. The various "facts" are arranged under the heads of the sciences to which they principally refer, but there is no sequence in the arrangement, nor is there added the few words of explanation, the date, or the circumstance, that in many cases would make the events more interesting. An obituary at the end of the book gives the names of some great men and the lives of others, of whom we confess never to have heard before. We have been unable to discover whether distinction in art, science, or literature has opened the portals of this Walhalla, in which, whilst others have a page or two, Maclise, Chas. Dickens, the Earl of Clarendon, and Professor J. Redtenbacher obtain one line each.

Report to the Board of Visitors of the Royal Observatory, Edinburgh, at their Visitation, held on Wednesday, the 29th of June, 1870, at Three, p.m. By C. PIAZZI SMYTH.

Papers on the Great Pyramid, including a Critical Examination of Sir Henry James's "Notes on the Great Pyramid of Egypt." By ST. JOHN VINCENT DAY, C.E., F.R.S.E, &c. Edinburgh: Edmonston and Douglas.

Plates and Notes relating to some special features in structures called Pyramids. By ST. JOHN VINCENT DAY, C.E., F.R.S.S.A. Edinburgh: Edmonston and Douglas.

IT needs no argument to prove to the scientific mind the necessity of accurate standards of measurement, and that even popular feeling is strongly enlisted in this matter is demonstrated by frequent Royal Commissions issued by the Government in answer to the call from without. Nor is this feeling of modern growth. The allusions to justness of weight and measure are frequent in the early books of the Jewish people, especially in the Book of Proverbs. One of the main claims on which the

soul of the dying Egyptian rested its hope of happiness in a future state of existence was, that he had "not shortened the cubit," whilst in that future state he expected to encounter as one of his first sights "the God Thoth with the cubit in his hand." Could one of the numerous mummies resting in the museums on British soil now come to life again he would no doubt recognise his god Thoth under the guise of the Astronomer-Royal for the northern part of this island, who has so manfully stood up for the accurate measurement of the old Egyptians' monument of metrology, the Great Pyramid of Cheops.

After all the time, trouble, and expense devoted by Professor C. Piazzi Smyth to determining the minutest details of this marvellous and mysterious monument of early engineering skill, and his persevering efforts to read the riddle of its construction, it must, to say the least of it, have been disappointing that when the Lords of the Treasury had refused "to entertain the question of the measurement of the Great Pyramid" suggested to them in a formal report, the work should have been undertaken by the Department of the Ordnance Survey as a *parergon*, a bye-work, a matter of secondary importance to the main object of the expedition which was to survey the Sinaitic Peninsula, and this without previous information to the man who had done most to draw public attention to the desirability of this enquiry. When, however, it appeared that the Director-General of the Survey had published notes and opinions upon the Pyramid itself, in which there was a considerable want of accuracy both of calculation and of statement of lengths, it was scarcely to be hoped that the results of the survey itself would be satisfactory to those most anxious for it, since those results were not the actual details of each individual measurement, but means arrived at by calculations made by the department and not fully explained.

Both Professor C. P. Smyth and Mr. Day devote a considerable portion of their papers to the refutation of Sir Henry James's calculations and deductions about the Pyramid, and as it seems to us convict him of inaccuracy in his earlier remarks, and thus throw a doubt over the results of the last survey. We hope this may lead only to a fuller publication of all the data upon which these results have been arrived at.

The Report of Professor C. P. Smyth contains other interesting matter, amongst which we would especially draw attention to the determination of the causes of variation in the transit observations at the Carlton Hill Observatory. It was for some time supposed that these originated in contractions and expansions of the rock upon which the piers of the instrument rested; but on comparing the mean variations of the instrument for ten years, with the mean variations of the temperature of the atmosphere, and also of the earth at various depths, it appeared that the extreme errors of the instrument preceded the supposed cause. Experiment then showed that the piers themselves were

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liable to considerable and irregular expansion, thus vitiating the most careful observations and corrections. The Report on this subject is most interesting, and is accompanied by plates showing the different variations. It is a striking example of careful observation and tabulation of facts leading directly to the discovery of the cause of the disturbance.

Mr. Day, besides his successful onslaught on Sir Henry James, gives an account of the various attempts to measure the Pyramid, and shows why the results of the French *savans* and of Colonel Howard Vyse were, until the last few years, the only ones on which reliance could be placed. In a paper printed at the end of his book, but written previously, Mr. Day attacks "the 'development' band of Darwins, Crawfurds. Lubbocks, *et hoc genus omne*" with language which is somewhat stronger than his arguments. It seems strange that people cannot carry on investigation in their own line without branching off into topics with which the one in hand has no clear connection.

The plates in Mr. Day's other work are most elaborately and carefully engraved, and no doubt will be exceedingly useful to those for whom they are intended, viz., those who are already tolerably well acquainted with the literature of the subject; and we agree with the author's opinion that the volume will be of little use "to those uninitiated in the peculiarities of that class of ancient structures which are in most cases erroneously called 'pyramids.'" For four of the fifteen plates we are indebted to Professor Piazzi Smyth, from whom the world has received an example rarely equalled of a devotion of extraordinary powers to the drudgery of scientific verification. Whilst theories of such momentous importance are in the balance, every contribution towards arriving at well-founded conclusions must be hailed with gratitude, and we cannot but thank those two writers for clearing away some of the difficulties in this remarkable investigation.

The Natural History of Commerce, with a copious list of Commercial Terms and their Synonyms in several languages. By JOHN YEATS, LL.D., &c., assisted by several scientific gentlemen. London: Cassell, Petter, and Galpin.

MUCH has been written and done in regard to the education of the poor—the Education Bill, like most Acts of Parliament, following upon greater activity outside the walls of the Legislature, merely supplements what it appears to have created. Much, also, has been done for the education of the rich, as is testified by deep and radical changes in our Universities, our Public Schools, and our Grammar Schools. At both ends of the scale of society endowments more or less permanent assist the work that parents themselves do not value sufficiently to pay for at the current market rate. But in the meantime the middle

classes are left without the excitement of discussion or the assistance of external provision to seek the education required as much for their sons as for those of the other classes of society. It is not surprising, therefore, if in their natural ignorance of the essential qualifications of teachers and teaching they should have suffered unduly from what, being put forward as education, has turned out to be an unreal imitation of mental training and a useless accumulation of disjointed facts. We hail, therefore, as a harbinger of better things such a work as the one under review. Written by a schoolmaster, it is intended not only for his boys whilst under instruction in school, but the information collected is especially designed to be carried away with them into their after work, and to be consulted and referred to when the *schooling* is over, but whilst the education is still going on. It is one of the main faults of much that is called education or instruction that it has no reference to the future pursuits of the learner, and, in consequence, the child which has an unreceptive brain loses that learning from never employing it. Whilst strongly deprecating a mere technical education, we still hold that the only way in which to make a liberal education act upon the whole character of the individual is to connect the general with the particular pursuits of the individual. We hope to see the new School Boards bearing this in mind, and providing that the children of the poor shall not only be taught those subjects which would fit them for a higher station in society than their fathers held, but that they shall also learn the principles of those labours to which their future life will be devoted. The present work brings before us this connection. It shows the application of geography, geology, meteorology, as well as the various branches of natural history, to the work of the counting-house and of the shop. Whilst, therefore, interesting the mind of the lad whose work is concentrated upon the supply of a certain class of productions, it leads him on to the circumstances attendant upon the growth and supply of these and similar substances, to the noting of differences, and to accounting for their causes; and whilst not distracting him from the work that is to furnish his bread, it enlarges his mind to the embracing of new facts, which will enable him to recognise and provide for new wants and new productions.

The plan of Dr. Yeats's book, of which the idea has been taken from the school books in use in the commercial schools of Holland, Belgium, and Germany, is as follows:—After showing the diversity of our own land and of its productions, he points out what countries are analogous to it in various points of temperature, soil, &c., thus leading on to the question, Whence are our raw materials imported? Then taking in turn the various classes of raw materials derived from the Vegetable, the Animal, and the Mineral Kingdoms, the locality of each, and the means adopted for its production, with an outline of the process of manufacture

are described. To this is added a table of the names of materials in a great variety of languages. The work seems to have been thoroughly done, and the information is up to the present day, whilst the style is readable and agreeable. We congratulate Dr. Yeats on having produced a book really useful for educational purposes,—adapted, too, for consultation when the work of life has begun, and also suited for more general reading. We are happy to see that the same author intends to carry on the work thus begun by an Industrial and Political History, and by a Technical History of Commerce, and thus to complete a series of works which will enable the young man engaged in commercial pursuits to connect his daily life with the literature which may really enlarge and inform his mind. Men thus trained would do much to wipe away the stigma so often cast at them by foreigners, that the British commercial man exists for the sole purpose of money getting without any higher or intellectual life.

Electrical Tables and Formulæ. By LATIMER CLARK and ROBERT SABINE. E. and F. N. Spon.

THE paucity of English telegraph literature gives rise to the greatest anticipation when any new work is announced, and when proceeding from the pens of two so well-known electricians and authors as Mr. Latimer Clark and Mr. Robert Sabine many a hope is expressed that it may be found adequate to the wants of the telegraph engineer. The existing literature would seem to be composed of two classes—works requiring a more than ordinary knowledge of elementary mathematics to render their application useful, and those in which the mere principles of the science are stated. The book under consideration is of the former class, though much useful information unembarassed by mathemetical formulæ will be met with in the sections devoted to the Laws of Magnetism, Electro-Magnetism, and Induction. It will be more in demand in the office of the chief-engineer than of use to the essentially practical operator. Exception must be made to the employment of the cube knot as a fundamental unit of resistance. A knot is a term occurring in but one branch of telegraphy, and even then the assumption prevents the verification of the formulæ by an individual inquirer. On the other hand, the insertion of the formula

$$I = \frac{E_1 + E_2 + E_3 + \ldots\ldots + E_n}{G + r_1 + r_2 + r_3 + \ldots\ldots + r_n},$$

as determining the current from a battery of n elements of differing electromotive forces and resistances connected in series, goes far to show that the authors have not been bound down by any erroneous ideas, however conventional. The joining-up of such a series would be considered by some rule-of-thumb operators

to produce a resultant electromotive force equal to that of the lowest cell only: it is these men who, from their deficiency in the mere elements of mathematics, will still be debarred the proof presented to them. The various units of electromotive force and resistance that have been or are extant are given with their equivalent British Association measures. The term *veber* is used to express the unit quantity of electricity, or that which passes through one *ohm*, in one *second*, with a difference of tension of one *volt*; while the *micro-farad* has been assigned as the unit of capacity. The tests given are from all sources and of all degrees of delicacy; the most delicate operation being, perhaps, that of eliminating the error due to the unknown temperature of the leading-in wires. Even though a new branch of engineering, telegraphy can boast in this work of a most compact collection of formulæ and tables. Every subject coming under the cognisance of the telegraph engineer has its chapter, whether it be the best method of winding galvanometer wires, or the course to steer a cable-laying ship, to take a set of soundings for a submarine cable, or to measure the timber for an overland line—in a word, the work is a dictionary for telegraph inspectors.

On some Disorders of the Nervous System in Childhood: being the Lumleian Lectures delivered at the Royal College of Physicians of London, in March, 1871. By CHARLES WEST, M.D., Fellow and Senior Censor of the College; Physician to the Hospital for Sick Children. London: Longmans, Green, and Co.

THE foundation of the Lumleian Lectures is intrinsically interesting in so far that they were founded by men who, in a remarkable time, were themselves most remarkable. In 1572, when Shakespeare and Bacon were boys, when Raleigh was serving his first apprenticeship to arms with the troops of the heroic Queen of Navarre, when religious dissensions were highest, and Europe struck aghast by the massacre of St. Bartholomew—Richard Caldwell, and John, Lord Lumley, executed a joint deed laying a perpetual rent-charge on their lands for the foundation of these lectures. Not only historical worth, but the fact that from the Lumleian chair William Harvey first publicly taught his doctrine of the circulation of the blood, will make the post one to be held only by the distinguished in the art of medicine. Dr. West has been the lecturer elected for this year, and, though he has no discovery to bring forward, the subject chosen will find favour with every parent. His book is the work of one who has passed much of his life among the young; to him their wants and sufferings are more familiar than those of grown-up people. The disorders of the nervous system have been selected, both because they are the most frequent and the most fatal, as also because their peculiarities are more

remarkable than those of any other class of diseases; for the nervous system is more informed, its functions more rudimentary, its condition one of change and development, the like of which does not take place in the organs of respiration, circulation, or even of digestion. In infancy and childhood we are told that pain referred to any part signifies almost without exception that disease of some sort or other is going on there, or near at hand. And then Dr. West proceeds to treat of neuralgia and epilepsy, chorea and paralysis, in a way that conveys information both to the medical man and to those having care of children.

Light Science for Leisure Hours. A Series of Familar Essays on Scientific Subjects, Natural Phenomena, &c. By RICHARD A. PROCTOR, B.A. Camb., F.R.A.S., Author of "The Sun," "Other Worlds than Ours," "Saturn and its System," &c. London: Longmans, Green, and Co., 1871.

THIS is a collection of essays selected from the author's contributions to serial literature during the past three or four years. Mr. Proctor tells us that his chief object has been to present scientific truths in a light and readable form—clearly and simply, but with an exact adherence to facts; and most strictly has he borne this view in mind. Devoid both of technicality and excessive simplicity, there is conveyed to the reader an immense amount of information. It is certainly a matter for most earnest congratulation that scientific men of the present day have lost that conservatism in their knowledge formerly so rampant; there is a desire to make known to others what is likely to interest, keeping back those dry details through which men, commercially engaged, have no time to wade. And this desire to extend the knowledge of others must in its reflex action bring beneficial results to the teacher, enlarging his auditory and perhaps his means of yet further instruction. It is such works as these from the pen of Mr. Proctor that silence the cry of *Cui bono*, unhappily so prevalent. We cannot all be scholars in the limited meaning of the word, but we may all be scholars in the school of Nature, learning to read what is present to all our senses. It is this that Mr. Proctor does—brings a cultured brain to sift those questions that others else have not the opportunity of even meeting with —a true interpreter, who learns Nature's language not for himself alone. Most of these essays have been before the public in another form, but there is not one that would pall upon a second perusal, while some, published in a college magazine, are new to the general reader. Beyond this the subjects are so various that he must be hard to please indeed who does not find sufficient to interest him.

Iron and Heat. By JAMES ARMOUR, C.E. London: Lockwood and Co. 163 pp.

THIS manual may be divided into two sections. The first comprises the fundamental principles concerned in the construction of iron beams, pillars, and bridge girders; the second deals with the action of heat upon the different materials of iron-smelting. The language is simple, and the mathematical illustrations in the first part of the work, as well as the definition of the laws of chemical combination concerned in the second, are such as will be readily understood by the practical workman. The use of logarithms in calculating strains by Hodgkinson's formulæ is shown with great clearness and brevity, while with equal terseness the combinations taking place chemically in the interior of the furnace during the smelting of the iron are explained by measure instead of by the atomic theory. Mr. Armour will no doubt find many readers among the practical men for whom he writes, and with whom his own experience will have its weight.

Fragments of Science for Unscientific People. By JOHN TYNDALL, LL.D., F.R.S. London: Longmans, Green, and Co.

THESE fragments are in reality solid intellectual repasts provided at different times for large audiences; but the illustrations are so fresh that the repetition consequent upon the aggregation of articles written on the same subject for distinct purposes does not cloy. The first essay is a very happy exposition of its title, "The Constitution of Nature." It presents to the unscientific reader the most important physical laws, pictured in that lucid manner which the author advises as the only successful method of study. Perhaps no other writer has been able to make the conception of an interstellar medium so comprehensible to the student. The present theory of Heat and Light is easily enough understood, once the imagination has accepted the existence of an interstellar luminiferous æther; but it is this endeavour to grasp the intangible that appears the stumbling-block of the tyro in science. How Dr. Tyndall makes this the starting-point from which he tends to the consideration of an extra-ætherial sun is best told in his own words.

"Imagine a paddle-wheel placed in water and caused to rotate. From it as a centre, waves would issue in all directions, and a wader as he approached the place of disturbance would be met by stronger and stronger waves. This gradual augmentation of the impressions made upon the wader's body is exactly analogous to the augmentation of light when we approach a luminous source. In the one case, however, the coarse common nerves of the body suffice; for the other we must have the finer

optic nerve. But suppose the water withdrawn, the action at a distance would then cease, and as far as the sense of touch is concerned, the wader would be first rendered conscious of the motion of the wheel by the actual blow of the paddles. The transference of motion from the paddles to the water is mechanically similar to the transference of molecular motion from the heated body to the æther; and the propagation of waves through the liquid is mechanically similar to the propagation of light and radiant heat."

But the power of this work most certainly culminates in the essay on the Scientific Use of the Imagination. Such an article is a true index of the liberality of present scientific investigation, and a true growth of the grand scheme of inductive logic. No reader of this essay can predict a limit to experimental philosophy, can say how soon electricity and magnetism may have supplied by a thoughtive imagination the missing links in the chain that will some day assuredly show their connection with the wave-theory of Light and Heat, binding the phenomena of nature in a comprehensive whole. And when it is seen what imagination has done for science, we cannot wonder that the pioneers of science acquire fresh energy to strike out new paths in the wilderness of fact. What the compass is to the explorer, Imagination is to the scientific investigator; both direct, but neither limit the distance, to be found only by successive trials. Most noticeable of all is the poetic feeling which the author brings to bear upon his subject. There is an emotional under-current that imperceptibly draws the student on to the more fatiguing paths of inquiry: a vividness of mental vision well illustrated, where it says, "The mind is, as it were, a photographic plate, which is gradually cleansed by the effort to think rightly, and which when so cleansed, and not before, receives impressions from the light of truth."

The Meteoric Theory of Saturn's Rings. By Augustus Morse Davies, B.A., F.R.A.S., &c. London: Longmans, Green, and Co.

This consideration of the Meteoric Theory results from seed sown by Mr. Proctor's "Saturn and its System," which induced the endeavour to think out the question of the accumulation of such an assemblage of minute attendants on this one member of the solar system. The work deals shortly and decisively with those elements of Saturn distinguishing it from the other planets, showing that, Jupiter excepted, Saturn from its extremely low velocity is most favourably circumstanced for attaching meteors in its train as satellites, particularly when the velocities of the meteors are considered. Then the distance of Saturn from the Sun exceeding that of Jupiter, Saturn's influence on passing meteors would be greater than that exerted by Jupiter,

the solar attraction varying according to the law of inverse squares. It does not appear strange that the meteoric theory has met with but few adherents amongst scientific men, when it is considered that the rings have been revealed by modern science alone. The fact that the god Nisroch, associated with Saturn, should be represented as a man encircled with a belt, does not go far towards an argument that the rings were known before the time of Galileo. The surprise would be why so few sound theories have been propounded to explain so unusual an attendance. But in this book, including, as it does, an epitome of the theories gone before, there is not much left to be desired. The meteoric theory can now most certainly boast of a clearly written manual, in which the argument is aided by sound analytical expressions and remarks drawn from the works of Sir W. Thomson, Herschel, Lyell, Arago, and Proctor. Appended is a paper on the Meteoric Theory of the Sun, in which the solar centre is considered to consist of a central globe, hollow or continuous, surrounded by a liquid envelope of molten metals. On this ocean rests an atmosphere of flaming gas, and this atmosphere is composed of volatilised substances from the liquid surface and the smaller meteors. In it the meteoric flights first become visible to us, in part, adding to its temperature by the resistance it offers to their motion, and showering down their substance in it. The pains taken to impart clear ideas certainly inspire the hope that the theory will be well accepted.

Select Methods in Chemical Analysis (*Chiefly Inorganic*). By William Crookes, F.R.S., &c. London: Longmans, Green, and Co., 1871.

It will be perceived from the title of this work that the author has not intended to provide the student with a complete text-book of analysis, but rather with a laboratory companion, containing information not usually found in ordinary works on Analysis. The book is essentially a reprint of the important articles on inorganic chemistry in the first twenty volumes of the "Chemical News," verified, condensed, and arranged in proper order; and as some of these have proved to be of great value, it was thought that a service would be rendered to analytical chemistry if these trustworthy methods of analysis were systematically arranged in a convenient form for laboratory use. In some instances the descriptions are given in the language of the original writer, but in all cases where the author has improved the processes, the necessary modifications have been introduced.

It is strange that modern works on analysis should ignore about twenty of the elements. Even Fresenius gives only a separate form for their detection. Were investigators more in the habit of looking for the "rare" elements, they would no

doubt turn up unexpectedly in many minerals. In this work equal prominence is given both to the rare and to the ordinary elements.

The order in which the analytical separation of the metals is carried out will be readily understood. Take, for instance, the case of copper. After giving the best method for the detection and quantitative estimation of this metal, comes a description of the processes for separating it from those metals which have been previously passed under review, as mercury, silver, and zinc; but no attention is paid to the separation of copper from such metals as lead, tin, &c., which have not previously been treated of. Under the respective headings "Lead" and "Tin," the separation of these metals from copper is described.

A complete list of separations has not been attempted. Where no process of separation or estimation is given, it may be inferred that the author has had no experience in any but the well-known methods employed in most laboratories; and to have introduced these ordinary processes into the work simply for the sake of filling up gaps would have largely increased its bulk without adding materially to its value. To save space, the description of a process is frequently discontinued at the point where the substances under separation are brought to such a state that the concluding steps are obvious.

No special system of weights and measures has been employed; many of the descriptions having been condensed from the original memoirs, it was thought better to retain the system therein adopted, so as to have simple numbers to deal with, instead of having to convert them to one common scale and to introduce decimals; thus—when an author says take 8 grains of a substance, 0·51816 gramme has not been substituted; and where 10 grammes are mentioned, he has not put 154·3840 grains. When not otherwise expressed, all degrees are according to the centigrade scale. Formulæ have been avoided as far as practicable.

A Series of Chemical Problems, for use in Colleges and Schools; adapted for the Preparation of Students for the Government Science and Society of Arts Examinations. By T. E. Thorpe, Ph.D., Professor of Chemistry in Anderson's University, Glasgow. With a Preface by Professor Roscoe, B.A., Ph.D., F.R.S. London: Macmillan and Co. Manchester: James Galt and Co., 1870.

In a short preface to this little book, Dr. Roscoe says that it will prove a great boon to those engaged either in teaching or in learning the science; and on going over the contents we perceive that the questions have been prepared with great skill, and

with special reference to junior classes in which elementary chemistry is taught. In order to give our readers some idea of the contents and manner of treatment, we quote the headings of the sections of this little work:—Chemical Calculations; French System of Weights and Measures; Conversion of Thermometric Scales; Correction of the Volume of Gases for Temperature and Pressure; Specific Gravity of Solids, Liquids, and Gases; To Calculate the Percentage Composition of a Compound from its Formula; To Calculate the Amount of Material required to Produce a Given Weight of any Substance: or the Quantity of the Substance produced by the Decomposition of a known weight of the Material; Combination and Decomposition of Gaseous Bodies; Calculation of the Results of Atomic Weight Determinations; Deduction of the Empirical Formula of a Body from its Percentage Composition; Calculation of the Formula of a Body from the Results of its Analysis; General Analytical Questions; Exercises on the Specific Heat, Latent Heat, Calorific Power, and Calorific Intensity of Substances. The Appendix contains in tabulated forms:—(1.) Combining weights and symbols of the elements; (2.) Weight of 1 c.c. of atmospheric air at different temperatures, from 0° to 300°, at 760 m.m. pressure; the weight of 1000 c.c. of water of $t°$ C., when determined by means of brass weights in air of 0° C., and of a tension 0·760 m.m., is equal to $1000-x$ grms.; volume and density of water at different temperatures (Kopp); for the conversion of the degrees (T′) of a mercurial thermometer into the corresponding values (T″) of an air thermometer; portions of tables of logarithms and antilogarithms. We sincerely wish that this excellent book, which is to be shortly followed by a key to the questions contained therein, will prove an assistance to teachers as well as pupils, and become a great impulse to a more general learning of a science which is, on account of its utility, of the greatest national importance to the whole community.

The Sub-tropical Garden, or Beauty of Form in the Flower Garden. By W. Robinson, F.L.S.; with Illustrations. London: J. Murray, 1871.

The term "sub-tropical" is scarcely a happy expression, and if this work had reference only to plants which required a sub-tropical climate to bring out their beauties, it would be of little use except to the very wealthy. This term is, however, here used in a wider sense, and it means the culture of plants with large and graceful or remarkable foliage or habit, and the association of them with the usually low-growing and brilliant flowering plants now so common in our gardens, and which

frequently eradicate every trace of beauty of form therein, making the flower garden a thing of large masses of colour only. The author informs us that his guiding aim has been the selection of really suitable subjects, and he has rejected several that have been recommended and tried for this purpose. Many have been excluded which have been long acclimatised in this country; likewise some very tender stove plants, which require too much care. But no tropical or sub-tropical subject that is really effective has been omitted. The illustrations are very numerous, and give a striking idea of the ornamental value in gardens of plants having large and handsome leaves or graceful port. Through the courtesy of the publisher we are enabled to illustrate our remarks with some of the cuts decorating this handsome volume.

One of the greatest mistakes ever made in the flower garden was the adoption of a few varieties of plants for culture on a vast scale to the exclusion of interest and variety, and too often of beauty or taste. To quote the words of our author, "We have seen how well the pointed, tapering leaves of the *Cannas* carry the eye upwards; how refreshing it is to cool the eyes in the deep green of those thoroughly tropical Castor-oil plants with their gigantic leaves; how grand the *Wigandia* with its wrought-iron texture and massive outline looks after we have surveyed brilliant hues and richly painted leaves; how greatly the sweeping Palm leaves beautify the British flower garden; and in a word the system has shown us the difference between the gardening that interests and delights all beholders as well as the mere horticulturist, and that which is too often offensive to the eye of taste."

It is a mistake to imagine that this kind of gardening can only be indulged in by those who possess conservatories, hothouses, or other means of preserving tender plants in the winter. What, for instance, can be more beautiful than the Pampas grass, which, when well grown, is unsurpassed by anything that requires protection. There are the *Yuccas*, noble and graceful in outline, and thoroughly hardy, and which if planted well are scarcely to be equalled by anything of like habits we can preserve indoors. There are the *Arundos conspicua* and *Donax*, things that well repay for liberal planting; and there are fine hardy herbaceous plants, like *Crambe cordifolia*, *Rheum Emodi*, *Ferulas*, and various graceful umbelliferous plants, that will furnish effects equal to any we can produce by using the tenderest exotics. The *Acanthuses*, too, when well grown are very suitable for this use. Among the *Yuccas* the *Y. pendula*, considering its graceful and noble habit, is simply invaluable in every garden, and when once planted, may be left year after year without protection. The accompanying illustration shows the grace and symmetry acquired by old and well established specimens standing alone on the grass, from the lower leaves which

sweep the ground, to the central ones that point up as straight as a needle.

But if the garden is furnished with conservatories properly heated for the winter, there is scarcely any limit to the beauty

FIG. 4.

YUCCA PENDULA.
Hardy evergreen, fine foliaged type.

which may be attained. What, for instance, can be more beautiful than the shady and sheltered dell figured on the next page, with tree-ferns and other stove plants placed out for the summer.

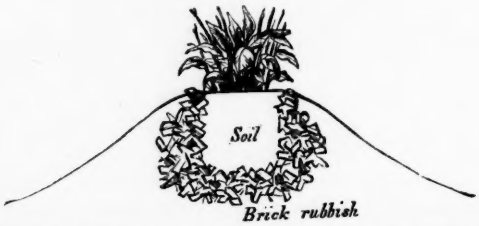
The author speaks with approval of a peculiar mode of preparing the beds for the finer sub-tropical plants as practised in Battersea Park. Here many of the beds are raised above the level of the ground, and underneath and around the mass of light rich soil is a good layer of brick rubbish, as shown in the

accompanying engraving (Fig. 6). The soil is first excavated, and thrown round the margin of the bed, then the brick-rubbish is put in on the bottom and around the sides also, raising the bed somewhat above the level of the ground; the cavity in the centre is then filled up generally with fine light rich soil, and the outside is arranged in a gentle slope, and covered neatly

Fig. 5.

with turf. The soil may vary in depth from 3 feet to 18 inches, according to the kinds of plants to be grown in it. The chief advantage which this method insures is that of good drainage, but it renders efficient protection against cold winds somewhat difficult. This latter shelter is a most essential requisite for

Fig 6.



all the more delicate kinds of plants. Mr. Robinson's remarks on this subject are very judicious, and will well bear quotation:—"Warm, sunny, and thoroughly sheltered dells should be chosen where convenient; and in any case positions which are sheltered should be selected, as the leaves of all the latter kinds

suffer very much from strong winds, from which they will be protected if judiciously planted near sheltering banks and trees. Even in quite level districts it will be possible to secure shelter by planting trees of various kinds; among which such graceful conifers as *Thujopsis borealis*, *Thuja gigantea* (true), *Cupressus macrocarpa*, *Cryptomeria elegans*, &c., should be freely used in the foreground, as in beauty of form they are unsurpassed by any short-lived inhabitants of the summer garden. Except, however, in the case of the Tree-ferns and various other things not grown in the open air, but simply placed there for the summer, it is very desirable not to place the plants in the shade of trees. All the things which have to *grow* in the open air should be placed in the full sun. Not a few hardy subjects will thrive very well without any but ordinary shelter, as, for example, the *Yuccas* and *Acanthuses;* but judging by the remarkable way in which the hardy Bamboo thrives when placed in a sheltered dell, shelter has a considerable influence on the well-being even of these, as it must have on all subjects with large leaf surfaces. But it should not be forgotten that shelter may be well secured without placing the beds or groups so near trees that they will be robbed, shaded, or otherwise injured by them."

The book is divided into three parts; the first contains Introduction and General Considerations; the second gives the Description, Arrangement, Culture, &c., of suitable species, hardy and tender, alphabetically arranged; whilst the third part contains Selections of Plants for various purposes in the Subtropical garden. The second part occupies far the largest portion of the book, and is of the greatest use, as it gives full descriptions of all (frequently illustrated with woodcuts) the plants which the author has selected, the kind of care they require, and the most suitable position in the Subtropical garden for them to be displayed to the best advantage. As an illustration of the method adopted by the author in this part of the work we cannot do better than give a quotation:—

"*Dicksonia antarctica.*—A very noble evergreen tree-fern, with a stout trunk, which varies considerably in thickness, and attains a height of 30 feet or more. The fronds, which form a magnificent crown 20 feet or 30 feet across, are lancet-shaped, much divided, of a shining dark green on the upper surface, and paler underneath, from 6 feet to 20 feet long, beautifully arched, and becoming pendulous with age. Perhaps the hardiest of tree-ferns, and therefore most suitable for placing in the open air in summer in sheltered shady dells, from the middle of May to the beginning of October." (See Fig. 7, on the next page.)

It is with mingled feelings of pleasure and regret that we close this work—regret that those who have had the laying out of some of our parks and public gardens have not adopted more freely the judicious views here expressed, in preference to

fatiguing the eye and offending the taste by the sight of hard, geometrical patterns, and glaring chromatic made up masses of blossom, variegated gravel, and box edging; and pleasure to

Fig. 7.

Tree-Fern.

For half-shady sheltered dells, in warmer and milder districts during the summer months.

think that the publication of such a book as this cannot fail to produce a beneficial effect, contributing as it does so many valuable suggestions in connection with an art for which Englishmen have long taken the highest rank,—that of landscape gardening.

PROGRESS IN SCIENCE.

MINING.

THE recent development of the extraordinary mineral districts in Nevada and Colorado, now rendered accessible by the Pacific Railroad, has led to an official exploration of the chief ore-bearing regions in these States. In 1867 Mr. Clarence King was appointed Geologist in charge of the expedition, and the volume of the Report which relates to Mining, prepared chiefly by Mr. J. D. Hague, has recently been published.* The complete report will comprise five volumes—two on Geology, one on Mining, and the others on Zoology and Botany. The third volume—that on Mining—has been issued first in consequence of its great practical importance. It forms a magnificent quarto volume amply illustrated, and accompanied by a large folio atlas of maps, plans, and sections, printed in colours.

Although professedly an exploration of the fortieth parallel, the survey has been extended to a belt of country stretching for about 100 miles from north and south, and included between the meridians of 109° and 120°; moreover, any important mining districts lying adjacent are also noticed.

Over the whole Cordilleras deposits of metallic ores are more or less abundant; these deposits usually range in longitudinal zones parallel to the general trend of the rocks, which is nearly north and south. The Pacific Coast Range upon the west carries ores of mercury, tin, and chrome-iron ore; the next belt —that of the Sierra Nevada and Oregon Cascades—bears on its western slope copper and gold, while along the eastern base is a chain of silver mines; these silver deposits extend into Mexico, whilst the gold ores—both auriferous veins and placer workings—run far into Alasca. Through Middle Mexico, Arizona, Middle Nevada, and Central Idaho is another range of silver-mines; and through New Mexico, Utah, and West Montana a zone of lodes containing argentiferous galena; whilst on the east a belt of gold deposits runs through New Mexico, Colorado, Wyoming, and Montana. For a proper understanding of these mineral deposits, it is of course needful to study the geology of the far West. A series of conformably stratified beds, varying in age from early Palæozoic to late Jurassic times, appear to have been at the latter period upheaved and corrugated, whilst masses of granitic and porphyritic rocks were injected into the fissures formed by this disturbance. Deposits of cretaceous and tertiary age were then laid down on the flanks of these rocks, and at a period subsequent to the miocene epoch they were folded into mountain-chains parallel to, but outside of, the earlier system. This second upheaval was accompanied by volcanic disturbances, the lavas overflowing and covering the earlier deposits. The mineral lodes of these regions consequently resolve themselves into two distinct sets, corresponding to the two periods of disturbance—firstly, those of Jurassic age, consisting of veins of granite, or in more or less metamorphic strata, varying in date from Jurassic to Devonian; and, secondly, veins of tertiary age in volcanic rocks. A large number of lodes are referable to the first date, but many of the richest, such as the Comstock lode, are referable to the second disturbance.

As might be expected, a large proportion of the volume is occupied by a description of the celebrated Comstock vein. This lode courses nearly N. and S., and has been traced for a distance of four miles, the part already developed occupying about two miles. The masses of ore occur in disconnected groups, and the width of the vein varies considerably, sometimes containing masses of vein-stuff 100 ft. in width. The report contains a minute description of the lode, with details of the many mines opened upon it, and of

* Report of the Geological Exploration of the Fortieth Parallel; vol. iii., Mining Industry, by J. D. Hague, with Geological Contributions by Clarence King. Washington, 1870.

the metallurgical treatment to which the Comstock ores are subjected. The mines of Central and Eastern Nevada, and of Colorado, also receive ample notice.

During the last few years the great development of the steel trade, due to Mr. Bessemer's improvements, has led to the extensive working of our deposits of red hæmatite, an ore which by its freedom from phosphorus yields an iron well adapted for conversion into Bessemer steel. Our deposits of this ore are, however, extremely limited, being confined chiefly to the mines in the carboniferous limestone of the neighbourhood of Ulverstone, in Lancashire, and Whitehaven, in Cumberland; and hence apprehension has not unnaturally been entertained that these workings may soon become insufficient to meet the ever-growing demand for hæmatite. Accordingly the Iron and Steel Institute, some time ago, appointed a committee to report on the distribution of our iron ores, with special reference to deposits of hæmatite, and at a recent meeting of the Institute this committee presented its preliminary report. It appears that an application was made by the Secretary to the Director-General of the Geological Survey, requesting that the officers serving under him should be instructed to report upon this subject. Whilst we feel assured that the Survey has ever been willing to further the mining interests of the country, it is obvious that the progress of a great national work of this kind can be interfered with only under very special circumstances. Hence we are hardly surprised that the Director-General does not feel justified in immediately acquiescing in the scheme of the Institute, although willing to do so at the earliest opportunity. A slight misunderstanding, however, appears to have been entertained on this subject, which elicited some explanatory remarks from Mr. Robert Hunt.

Those who are interested in the promotion of mining education in this country will have learnt with surprise that the Royal Commission on Science has suggested in its Preliminary Report, that the Royal School of Mines and Mining Record Office shall be separated from the Museum of Practical Geology, and that the School be removed to South Kensington. However ardently we may desire to see the establishment of a grand central College of Science comparable with some of the continental institutions, the policy of the proposed step with reference to the Mining School may be fairly questioned. Such technical subjects as mining and metallurgy should obviously be taught in an institution where the student may readily gain access to good collections of minerals, metallurgical products, and mining models. Nowhere can these conditions be better fulfilled than in Jermyn Street, where for the last twenty years valuable collections have been gradually accumulating. Indeed, it is certain that many of the objects—such as the grand series of vein-stones—could not possibly be obtained in duplicate for Kensington. Nor must we ignore the convenience to the public arising from the concentration of the Mining School, the Geological Museum, the Mining Record Office, and the head-quarters of the Geological Survey under the same roof.

Three valuable memoirs on mining subjects, by Mr. W. Jory Henwood, of Penzance, form the eighth volume of the "Transactions of the Geological Society of Cornwall," recently published—a volume which is divided into two parts, each forming a stout octavo. Mr. Henwood's first memoir occupies between 700 and 800 pages, and describes in detail more than 130 mines in India, Chili, Brazil, North America, Jamaica, Spain, France, the Channel Islands, and Great Britain and Ireland. These lengthy "Observations on Metalliferous Deposits" are followed by two shorter papers—one giving the result of certain "Observations on Subterranean Temperature," and the other "On the Change of Temperature which takes place, at the same and at different times, on the Surface and at Depths of Three, Six, and Nine feet in the Canga at Agoa Quente, in Brazil." The present volume is a befitting companion to the fifth volume of the same series, which is devoted to Mr. Henwood's celebrated "Memoir on the Metalliferous Deposits in the West of England."

We have before us the last Report of "The Miners' Association of Cornwall and Devonshire," which shows what useful but unpretending work the associ-

ation continues to effect. In an introductory notice, the Honorary Secretary, Mr. R. Hunt, F.R.S., sketches the rise and progress of the association, and shows the part which it has played in supplying the working miner with sound, scientific education. It is really vexing to learn that such good work should be impeded by insufficiency of funds. The present report contains papers on boiler explosions in Cornwall, by Mr. C. E. Martin; on the use of Haupt's improved rock-drill in Swaledale, by Sir G. M. Denys, Bart.; and on a new buddle for dressing gold, tin, copper, lead, or other ores, by Mr. R. H. Williams.

A paper on the iron-producing districts of Central Sweden has been communicated to the Iron and Steel Institute, by Mr. David Forbes, F.R.S. The disadvantageous inland position of these mineral deposits has hitherto impeded their development, but the construction of a railway traversing the heart of these iron-mining districts will probably render them available to the English iron-master, whilst a considerable impetus may be given to the iron-trade of Sweden, which of late years has been decidedly on the wane.

METALLURGY.

Considerable discussion has lately arisen as to the value of the "Sherman Process" for purifying cast-iron. The application of very small quantities of iodine, or some compound of this element—such as potassic iodide—is said to effect the removal of sulphur and phosphorus from the crude metal, so that good wrought-iron or steel may be obtained from inferior brands of pig-iron. Mr. Snelus, of Dowlais, has, however, examined samples of steel treated by this process, and fails to detect any appreciable chemical difference between this steel and that made by the ordinary method. In defence, Mr. Sherman has published an analysis of a cast-steel said to have been made from Cleveland pig by his process. It is well known that the Cleveland iron contains a large proportion of phosphorus, but this is reduced to 0·04 per cent in the steel. The following analysis is due to Mr. Barker, of the Atlas Works, Sheffield:—Combined carbon, 0·67; graphite, traces; phosphorus, 0·04; sulphur, traces.

Mr. Bessemer suggests that if iodine is so efficacious in refining iron, it would certainly be more economical to employ crude substances containing iodine, such as vitrified sea-weed, than to use the element itself or its pure compounds.

Under the name of "Burnt Iron," our workmen recognise iron which has been damaged by re-heating or by exposure to excessive heat after balling in the puddling-furnace. Such iron is brittle, and having lost its fibrous character, breaks with a short crystalline fracture. The cause of this change has not hitherto been satisfactorily explained, but Mr. Mattieu Williams has presented to the Chemical Society a paper on this subject, in which he shows that the burning of iron is simply a process of oxidation, which, however, affects the whole substance of the metal. By working the iron in a reducing atmosphere, this internal oxidation is prevented, and the various devices employed by workmen to prevent burning are based upon this principle.

Steel raised to a yellow or white heat and suddenly cooled, is rendered permanently hard and brittle, and incapable of being tempered by simple re-heating. This "burnt-steel" presents a coarse granular fracture, and in addition to a crystalline structure shows certain rounded facets, with concavities known as "toad's-eyes." The carbon in steel prevents the oxidation which produces burnt iron; but it is known that a slow combustion of the carbon in steel may take place even at a low red heat, and that this action gradually proceeds inwards. Mr. Williams believes that the carbon-monoxide produced by interior oxidation may remain in occlusion in the iron, and that if steel be suddenly cooled, so that this occlusion is prevented, the minute bubbles of carbon-monoxide and oxygen thus arrested would render the steel porous, and so affect the cohesion of its molecules in such a way as to produce the characters of burnt steel.

Herr H. Scheerer publishes in the "Berg- und Huettenmaennische Zeitung"

the results of his examination of a crystallised Bessemer slag from Hoerde. Drusy cavities in the slag run out from the converters, often contain well-formed crystals isomorphous with rhodonite, pajsbergite, and babingtonite, and certain triclinic minerals of the augite group. The slag is a bisilicate of iron and manganese, and contains—Silica, 44·73; ferrous oxide, 20·59; manganous oxide, 32·74; lime, 1·53; and magnesia, 0·17. Its specific gravity was 3·08.

ENGINEERING—MILITARY, CIVIL, AND MECHANICAL.

Military Engineering—Field Guns.—The report has recently been published of a committee which was appointed in July last for the purpose of carrying out certain competitive trials, with the view of testing the relative efficiency of muzzle and breech-loading guns for field service. Two batteries were placed at the disposal of the Committee for the purposes of their experiments, one consisting of three 12-pounder and three 9-pounder breech-loading wrought-iron guns, and the other of six 9-pounder muzzle-loading bronze guns. Each gun was provided with 240 rounds, consisting of a certain number of rounds of shrapnel, segment, and common shell, of case shot, and of percussion and wood time fuzes. From the results of these experiments it appears, that in respect of shooting, the 9-pounder muzzle-loader and the 12-pounder breech-loader are about on a par, whilst the 9-pounder breech-loader is inferior to both. The advantages of simplicity, facility of repair, ease of working, rapidity of fire, original cost, and cost of maintenance, are all in favour of the muzzle-loader, whilst the breech-loader possesses the advantage of affording a superior amount of cover to the detachments when entrenched and in the open. The conclusion of the Committee is that, on the whole, a muzzle-loading gun is more efficient for war purposes, and they recommend that, if adopted for home service, they be made of wrought-iron with steel tubes. The report has been adopted by the Secretary of State for War, and it may therefore be concluded that the contest, so far as this country is concerned, has, for the present, been definitely settled in favour of the muzzle-loader.

Marine Heavy Ordnance.—The following description of our new 35-ton gun is taken from "Engineering." The great gun consists of six distinct parts; First, the inner or A tube, which is of Frith's steel, and is 13 feet 6½ inches in length, internal measurement. The thickness of the tube is 3½ inches at the breech, and tapering down to 2½ inches at the muzzle. Next this is the muzzle or B tube, which is of wrought-iron, and is shrunk on to the steel tube. The third piece forms the first-stepped joint, about 6 feet from the muzzle, and is known as the intermediate coil; it holds the front end of the coiled breech-piece which forms the fourth part. The fifth part is the outer or C coil, with its trunnion band, which is shrunk on to the breech portion of the gun, binding the rear end of the intermediate piece. The sixth and last piece is the cascable, which is made button-shaped instead of the ordinary form, in order to obtain the greatest possible length of bore in the gun within a restricted dimension, the Admiralty having limited the length over all to 16 feet 3 inches. The vent is placed on one side of the breech piece in order to render it easily accessible for firing. The gun is rifled to a calibre of 11·6 inches, and the length of the bore is 13 feet 6½ inches. The rifling consists of nine grooves, and in twist uniformly gaining from zero at the breech to 1 turn in 35 diameters at the muzzle. The weight of the projectile which this gun is intended to carry is 700 lbs.; the length of the solid shot being 30 inches, and that of the common shell 40 inches; the shell carries a bursting charge of 38 lbs.

Breech-loading Rifles.—On the 27th April last, a paper was read before the Institution of Mechanical Engineers, by Mr. W. P. Marshall, on "The Principal Constructions of Breech-loading Mechanism for Small Arms, and their Relative Mechanical Advantages; Illustrated by Specimens of Breech-loading Rifles." Mr. Marshall stated that breech-loading guns were first adopted by the Prussian army: the celebrated needle gun having been in use for more

than 20 years, while for many years it has been the universal arm used by that army. Although breech-loading carbines had been in use for 14 years, to a limited extent, for the cavalry of England, it was only since 1866 that the principle had been adopted for the army generally. Breech-loading carbines were adopted for the cavalry solely for the purpose of overcoming the difficulty of loading whilst on horseback. The writer after having described the various breech-loading guns, including the needle-gun, the Chassepôt, the Albini-Brændlin, the Werndl, Henry's, Soper's, Westley-Richards's, Sharp's, the Snider, Remington's, the Berdau, &c., went on to consider the Martini-Henry breech-loading gun, which has been selected for adoption in the British army after a long investigation and practical trial. We shall give a full description of this weapon upon a future occasion.

Marine Engineering—Docks.—The Somerset Dock at Malta was officially opened on the 16th of February last. Shortly after Malta came into the possession of the British Government in 1814, steps were taken to provide dock accommodation there. The site chosen was the Dockyard Creek, a small inlet of the great harbour. In the commencement a small graving dock was started on the south side of the creek, but the fissures in the rock permitted so much infiltration of water that it had to be abandoned. In 1840 the so-called old dock was commenced. It was completed in 1848, and in 1857 it was lengthened. The accommodation available at the dockyard was, however, at this time, very inefficient, and it was ultimately determined to construct a new dock, which should be of sufficient dimensions to receive the largest ironclad. The site chosen for the dock is immediately under St. Michael's bastion, where in 1565 the Turks conducted the obstinate and long-maintained siege of the towns of Senglea and Borgo. Before the works were commenced about 300,000 cubic yards of old materials, buildings, earth, and rock had to be removed. This occupied a considerable time, and it was the end of 1866 before the dock itself was commenced. The dimensions of the old and new docks are as follows:—

	Somerset Dock.	Old Dock.
Depth to floor	34 feet	25 feet.
Width between copings ..	104 „	82 „
Length on floor	430 „	301 „
Width of entrance ..	80 „	73 „

Considerable difficulties were experienced in making the excavations, numerous fissures having been encountered through which flowed large quantities of water. The smaller of these were stopped before the masonry was laid; in the larger ones cast-iron pipes were laid and the water carried off until the masonry was completed, after which the pipes were closed. The stonework of the dock is laid throughout upon the solid rock, the backing being of an inferior quality of the crystalline limestone of the island, and the floor and sides of a superior quality of the same stone. The thickness of the floor is 7 feet 6 inches. Of the first quality of stone 340,000 cubic feet were employed, and of the second quality 190,000 cubic feet. The entrance to the dock is closed by a sliding caisson, which runs back into a tunnel excavated in the rock beneath the Senglea bastion. The engine house contains two 60-horse power engines, which drive three centrifugal pumps, each of which throws a stream 3 feet in diameter.

A paper containing "An Account of the Basin for the Balance Dock and of the Marine Railways in connection therewith, at the Austrian Naval Station at Pola, on the Adriatic," by Mr. Hamilton E. Towle, of New York, U.S., was recently read before the Institution of Civil Engineers. It was at first intended that excavated docks should be formed, but in consequence of the volcanic and treacherous nature of the ground, this idea was found to be impossible of execution. A floating dock, basin, and railway system was therefore decided upon, the dock adopted being that known as Gilbert's Balance Floating Dock. The dimensions of the Basin are as follows:—

	Feet.	Inches.
Width inside the enclosing walls	211	6
Length	311	6
Depth from the top of the walls to the stringers in the floor of the dock	17	1½
Depth from the level of ordinary high water to the top of the stringers	13	0
Depth from the level of ordinary low water to the top of the stringers	11	0

The material selected for the wall of the basin was Santorin béton, composed of Santorin earth, a volcanic product from the Greek island of Santorino, and lime paste, in the proportion of 7 to 2, forming the hydraulic mortar; to this was added 7 parts of broken stone, the mixture being made into a conical heap, and tempered by exposure in the open air from one to three days, when it was ready for use. Of this béton extensive wharves and moles had already been constructed at Trieste, Fiume, and Pold, and as it had been found durable and efficient, was moderate in cost, and obtainable in any quantity, it was considered that no better material could be determined upon for the walls of the Pold basin.

Propellers.—On the 18th of April a paper was read at a meeting of the Institution of Civil Engineers, by Sir C. F. Knowles, Bart., M.A., F.R.S., "On the Archimedean Screw Propeller, or Helix of Maximum Work." In considering the defects of existing screw propellers, the author was led to propose to himself the problem, "What is the form of the surface of the screw propeller of which the *work done* is the greatest possible?" Referring the required surfaces to three rectangular co-ordinates, x, y, and z, one in the axis of rotation, the other two in the plane of rotation, the author first obtained a general expression for the total "work done" by the blade in propelling the ship, in the form of a double integral in terms of the co-ordinates x and y, and of the partial differentials of z with respect to each of them, of the speed of rotation of the blade, and lastly, of the speed of the ship. As this integral was to be a *maximum* for all points of the surface sought, it must be treated by the known methods of the calculus of variations. This done, an equation of condition was obtained, which, by the performance of the operations indicated by the symbols, led to an equation involving two factors, each factor being a partial differential equation between the three co-ordinates of the surface. The first of these being integrated gave for its solution the whole family of ordinary helices which were the surfaces of *least* work. The second factor was the differential equation of the required surface, the treatment of which was given in the paper *in extenso*. It led at once, and very simply, to an equation analogous to that of the common helix $\left(\tan.\ \theta = \frac{a \tan. a}{r}\right)$ viz., $\tan.\ ^2\theta = \frac{a \tan. 2a}{r}$. From this it was at once deducible that the surface of the blade at the axis cut the plane of rotation at an angle of 45°, while the common helix cut it at 90°, and therefore acted powerfully in the dead water to propel the ship, just where the common helix had no propelling power. It was proposed to call this surface the hemi-helix, or hemi-angular helix. The paper then proceeded to determine the pressure of this blade upon the vessel in the direction of the keel, and thence the whole circumstances of the ship's motion.

Civil Engineering.—The Kistnah Viaduct, now in course of construction in connection with the Great Indian Peninsula Railway, consists of 36 spans of 100 feet clear at the top of the columns, and is 3848 feet long from centre to centre of the end columns, which are built into the abutments. The piers are arranged for carrying a double line of railway, although at present girders for a single line only are erected. The section of the river is irregular, the height of the piers varying from 34 feet to 76 feet 6 inches from base of pier to rail level. The river bottom is hard rock, into which the cylinders of the piers are sunk, and to which they are securely bolted. The piers each consist of two columns formed of wrought-iron cylinders, averaging 10 feet in

diameter at the base, and tapering to 7 feet in diameter at the top. The vertical joints are formed internally of T irons, which are continuous from top to bottom, and are rivetted through to outside vertical strips. For the horizontal joints the ends of the cylinders are planed, the joints being butt-joints with strips inside and out. The cylinders are filled in with cement concrete, and finished with stone caps, which form the bases on which the bed girders rest, the caps being placed directly on the concrete. The bottom lengths of the cylinders are held down to the rock by eight bolts let head-downwards into holes drilled in the rock, which are afterwards filled in with Portland cement. Each pair of columns which form one pier are connected at the top by two transverse bed girders, on which the main girders rest on chairs. The main girders are on the Warren principle, each 103 feet long, and have a vertical depth of 9 feet 11 inches from centre to centre of pins. These girders are each fixed at one of their ends over alternate piers, the other ends being free to move on Bessemer steel rollers. The total weight of the ironwork in the superstructure of the Kistnah Viaduct is 2500 tons, whilst that in the cylinders is 1200 tons.

The *Czernowitz Bridge* crosses the river Pruth at the town of Czernowitz, the capital of the district of Bukowina, which forms the easternmost portion of the Austrian dominions. The bridge carries a roadway and two side footpaths, and measures 762 feet 6 inches in length, and 25 feet in width from centre to centre of girders. It has six openings over the river, each 126 feet between centres of piers; the five piers and two abutments are of masonry, resting on concrete foundations. The main girders are continuous, of the single lattice type, and are 11 feet 10 inches deep. The flanges are boxes composed of two large channel irons and a flange plate which connects them. The diagonals are placed at an angle of 45°, and consist of a pair of flat bars which form the ties, and a pair of channel irons braced together, which form the struts. Except at the piers, the main girders have no verticals, nor are they anywhere braced across the top flanges. The method adopted in order to prevent the top flanges from collapsing has been to carry them at every pier in two strong U-shaped frames, which keep the top flanges steady at this point, whilst the stiffness of the flanges between the piers is secured by their great width. The two main girders rest upon roller bearings at each of the piers except the centre one, so that any expansion from increase of temperature radiates outwards from the centre, and extends the bridge equally at each end. The roadway of the bridge consists of a timber platform carried on the cross girders, and supporting longitudinal timbers, upon which is laid transversely 4½-inch planking, and upon this again rest oak blocks 5 inches thick. The footpath is laid with 3-inch oak longitudinal decking, upon which the wearing planks are spiked.

Gas Engineering—Cleland's Steam Jet Gas Exhauster.—At a recent meeting of the Liverpool Polytechnic Society, Mr. William Cleland, Manager of the Lindere Gas Works, read a paper descriptive of the system of exhausting and washing gas by steam which he has adopted. This invention is named the direct action method of exhausting and washing gas by steam, because it aims at superseding in these operations the use of steam engines, and of exhausting and pumping machinery, by bringing the steam into actual contact with its work. The retorts used in the destructive distillation of coal and cannel being made either of coarse fire-clay or of iron, and both kinds becoming very leaky by use, there would thus be a serious loss of gas, if means were not used to prevent it. One of these means is exhausting, pumping, or sucking the gas from the retorts. It is well known that a current or stream of any fluid, moving through any fluid medium, produces, or tends to produce, around itself a current in the fluid medium. For instance, if we blow from the mouth through a small tube which projects into a larger tube open at both ends, the interior stream of breath sets in motion the whole body of air in the outer tube, causing at the further end a copious outflow, and at the near end an inflow. This effect has been termed lateral induction. The outer current is named the induced

current, the exterior tube the induction tube, and the interior tube the projector. Substitute for the exterior tube filled with air a gas main of the proper diameter filled with gas, communicating through the outlet with the gasholder, and through the inlet with the retorts; substitute for the central jet of breath a jet of high pressure steam, and you have at once a working gas exhauster complete in all its essentials. In practice, a projector with orifice ⅛ inch diameter, and a cylindrical induction pipe 3 feet long and 4 inches diameter, have been found sufficient to pass 50,000 cubic feet of gas per hour against a back pressure equal to 13 inches of water, and to maintain a steady vacuum of 2 inches in the hydraulic main.

Mechanical Engineering—Testing Rails.—The importance of instituting an efficient test for rails has been brought before the notice of the Institution of Civil Engineers by Mr. James Price. There are certain causes of increased wear in rails which can be avoided; first, the difference in the forms of rail-tops on lines worked as one system, the tyres which run on one portion not fitting the form of rail-top on other parts; and, secondly, the super-elevation of the outer rail on curves not being properly attended to. To facilitate calculation in the latter case, the author had invented a rule which left out the term radius—a chord being found, the versed sine of which was the correct super-elevation for any curve. This chord, for a speed of 40 miles per hour, was 64 feet for the English gauge, and 67 feet for the Irish gauge, a chain length (66 feet) being sufficiently near for either. For any speed and any gauge the rule was—"Length of chord whose versed sine equalled super-elevation = ¼ velocity in feet per second $\times \sqrt{\text{gauge.}}$"

The qualities sought in a rail are fourfold:—1. Strength as a girder, to sustain a moving load; 2. Toughness, to resist sudden strain or impact; 3. Solidity to resist separation under pressure; and, 4. Hardness, to resist wear of the surface. The flange, or Vignoles rail, stands first as to form, for strength, and the double-headed rail comes next, while the bridge rail is a bad girder form, and 30 per cent of such rails break before they are worn out.

Existing modes of testing rails may be divided into—1. Dead weight test, which is valueless as not being analogous to actual work; 2. Falling weight test, which is unfair, serviceable rails being rejected by it, as it goes as far beyond the requirements of practice as the former test falls short of it; 3. The examination of fracture to ascertain structure is a matter of too great nicety for general use. The machine used by the author for testing rails subjects them to wear analagous to actual use. It consists of a pair of metal rollers, 5 feet in diameter, 16 inches wide, and weighing 2¼ tons each, supporting a circular frame or beam weighing 6½ tons; this frame being connected by radii with a centre boss, through which passes a vertical axis. The circle traversed is 40 feet in diameter, and the pressure borne by the rollers is 6 tons and 5 tons respectively. Motion is communicated by a steam-engine, and the machine can be worked at about 20 miles per hour; but it is generally run at a speed of about 13 or 14 miles per hour. The rollers are caused to revolve over a ring or polygon of the rails to be tested until they are broken or worn out, and they bear with a weight equal to that of the driving wheel of a locomotive.

Sea Waves.—At the Friday evening meeting of the Royal Institution of Great Britain, May 26, 1871, Dr. W. J. Macquorn Rankine, C.E., F.R.S., delivered a most interesting lecture on this subject. In the first place he gave a summary, illustrated by diagrams and machines, of existing knowledge of the mode of motion of water in waves, and of the geometrical and dynamical laws which govern the relations between the depth of disturbance of the water, the velocity of advance of waves, their periodic time, and their length. He referred to the experimental and theoretical researches of previous authors on the subject, such as the Webers, Airy, Scott Russell, Caligny, &c. He then explained the principle, of which Mr. Froude was the first to point out the importance, that the action of water agitated by waves upon a ship tends to make her perform the motions which would have been performed in her absence by the mass of water that she displaces. In still water,

the forces of gravity and of buoyancy tend to keep the ship upright, and if she has been heeled over, to restore her to the upright position, and that tendency constitutes the *statical stability* or *stiffness* of the ship. Amongst waves the same forces, combined with the reactions due to the heaving motions of the water and of the ship, tend to place her in the position called *upright to the wave surface;* that is, with her originally vertical axis normal to the wave surface. If the ship yielded passively to that tendency, like a broad and shallow raft, she would accompany the waves in their rolling; and thus, a ship having great *stiffness* may be very deficient in *steadiness*. Every ship has, like a pendulum, a natural period of rolling, depending on her stiffness, or tendency to right herself, and her moment of inertia, being a quantity depending on the distribution of her mass. Stiffness tends to shorten, and inertia to lengthen, the period. It was shown in 1862, by Mr. Froude, that the greatest unsteadiness and the greatest danger of being overturned take place when the periodic times of rolling of the ship and of the waves are equal; for then each successive wave adds to the extent of roll; and if the coincidence of the periods were exact, the ship would inevitably be overturned in the end.

In the course of the present spring it has been pointed out that in well-designed ships a safeguard exists against the occurrence of such disasters. It is well known that no pendulum is absolutely isochronous; but great oscillations occupy a longer time than small oscillations. In like manner, no ship is absolutely isochronous in her natural rolling; but great angles of roll occupy longer periods than small.* Hence, supposing a ship to encounter waves of a period equal or nearly equal to her own natural period for small angles of roll, her angle of rolling is at first progressively increased; but at the same time her natural periodic time of rolling is increased, until it is no longer equal or nearly equal to the periodic time of the waves; and thus she in a manner *eludes* the danger arising from coincidence of periods. In order, however, that this safeguard may act efficiently, it is essential that the natural period of the ship for the smallest angles of roll should not be less than the period of the waves; otherwise the first effect of the progressive increase of angle will be, not to destroy, but to produce coincidence of period; and the result will be great unsteadiness of motion, and possibly great danger.*

The speaker described the above principles as being the latest additions to our knowledge of the theory of the relations between ships and sea-waves; and he illustrated them by means of experiments on a machine so constructed as to imitate the dynamical condition of a ship rolling amongst waves.

ANIMAL MECHANICS.—A few weeks ago the Rev. Samuel Haughton, M.D., D.C.L., F.R.S., of Dublin, delivered a series of three Tuesday Afternoon Lectures at the Royal Institution, on "The Principle of Least Action in Nature, illustrated by Animal Mechanics."

Dr. Haughton said that Alphonso Borelli taught Mathematics in the University at Naples towards the close of the seventeenth century; he was also Professor of Anatomy at the University, and his book shows that his scheme of uniting Anatomy with Geometry had the honour of being approved by the Pope, and pronounced to contain nothing dangerous to faith or morals. This book was published in 1680 under the title of "De Motu Animalium." It is full of mistakes because of the want of knowledge of the author of the composition of forces discovered by Newton; but with all its defects it is the only book which can be called a systematic scientific treatise on Animal Mechanics. Two Germans, Edward and Wilhelm Weber, of Göttingen, tried in later times to unite Anatomy and Geometry, and between them produced a very good

* An exception to this rule exists in the case of that form of ship known as the "Symondite," in which the sides flare out at and near the water-line, so as to make the stiffness increase faster than the angle of heel. In such ships the period of rolling *shortens* when the angle increases; and thus the well-known unsteadiness of large vessels of that model is accounted for. In a small boat, whose natural periodic time for the smallest angle of roll is shorter than that of any of the waves she encounters, the Symondite model does not promote unsteadiness; for the shortening of the natural period of rolling removes it farther from coincidence with the period of the waves.

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treatise on the motions of the human body, but the work is a less systematic and complete one than that of Borelli.

The lecturer said that he would explain what he meant by "the Principle of Least Action in Nature," by the aid of a few illustrations. Let the earth be supposed to be a lazy animal swimming round the sun in a curve which would enable it to complete the journey with the least trouble and exertion to itself; on this assumption, if two certain points in the path of the planet be known, also the position of the sun, the whole of its path can be accurately calculated and predicted. If a ray of light on striking and being bent from its original path by a piece of glass be supposed to be a living intelligent animal trying to perform its journey through air and glass at the least trouble to itself, on this hypothesis its path can be calculated and predicted, just as easily as by the laws of refraction and reflection. Some time ago he saw some oyster women at the Mumbles Harbour, near Swansea, carrying their oysters along a road which consisted of two parts; there was the slippery shingle of the beach, and beyond that the smooth common. The velocity of these poor women on the shingle and on the common was different. They did not go straight from the shingle to the common as he would have done, but "made a tack;" he afterwards measured the angles made by their path, and made a calculation which proved to him that they had unconsciously taken the path of minimum trouble. Lastly, he cited the instance of the cell of the bee, in which the largest quantity of cell-space is obtained with the minimum quantity of wax.

Dr. Haughton then stated that before proceeding to apply this principle of least action in nature he found it to be necessary to obtain the coefficient of muscular force. What is known to engineers as the coefficient of a rope, means the number of pounds or tons weight necessary to break it across. If a rope of muscle one square inch in cross section were hanging from the roof of the theatre of the Royal Institution, and that muscular rope were acted upon by the will, its coefficient would be the weight it would lift from the ground by contraction. It cost him twelve years of hard work to obtain this coefficient in pounds per square inch for human muscle. He had not succeeded in obtaining it for any other animal but man, the hairy quadrupeds with long tails not being intelligent enough to submit to the necessary experiments. He had determined that 94·7 lbs. per square inch of muscular fibre was the weight which the arms of a young man accustomed to athletic exercises was capable of lifting; 110·4 lbs. was the coefficient of the muscular fibres of the legs; and 107 lbs. of the muscles covering the abdomen; consequently 104·04 lbs. was very nearly the coefficient of muscular force.

He had obtained this information by dint of hard study of cases of cholera, hydrophobia, and lock-jaw in hospitals, in which very painful contortions sometimes occur, but which threw light upon some of the problems in animal mechanics. He had also learnt the secret of working the treadmill in a lazy manner, and could get through the work with the least trouble to himself as well as the most expert burglar in London; he also knew where to place himself to the best advantage on the wheel. Some of these trade secrets he unlocked in the first instance from the hearts of their possessors by means of an ounce of tobacco; he must say that burglars and thieves were much better people than he once believed them to be, and he thought that more good would be done by treating them with kindness than with severity. He had not only to ascertain the power of each particular muscle during life, but to measure its dimensions after death; but if he measured the cross sections of the muscles of aged people after lingering illness, false results would have been obtained. The cross section of a muscle during life is much larger than after death. So he had to watch for cases of severe accidents in the hospitals, but if a man died a violent death the cause was usually so plain, that the coroner would not order a medical examination of the body, and the friends of the deceased were always in a hurry to "wake" him. Those in Dublin who were executed by the hands of the law were nearly always patriots who had shot their landlords, so they had a vast amount of popular sympathy on their side, and it would have been dangerous for any scientific man to dissect their

bodies. All these things placed many difficulties in his path. At last a brilliant idea flashed across his mind, which he was compelled reluctantly to set aside, namely, to take a farm in Westmeath, refuse to pay his rent, shoot his landlord, and dissect him at his leisure. He believed, upon his honour, that public opinion in Ireland would not sanction the shooting of a landlord in order to obtain the coefficient of muscular force. But he had overcome the difficulties in his investigation, and had hoped to show that the tendons of the legs and arms of animals were constructed on the principle of least action in nature, as if every one of the muscular organs were itself instinct with life and reason. He added, "It is not the instinct of the planet, or of the oyster woman, or of the bee, that guides them in their path. There is instinct, there is knowledge, there is foresight, there is calculation; it is the wisdom, the knowledge, the foresight, and the calculation, of the great Architect and Geometrician of the Universe."

Dr. Haughton next called attention to the amount of friction in the hands and feet of various animals, and the strength of the various tendons. He said that the animals whose tendons suffer least from friction at the wrist and ankle are the goat and kangaroo. In the wrist of the goat no force whatever is lost, and the most perfect organ of locomotion he was acquainted with among animals was the hind leg of the boomer kangaroo. He also explained why, in certain cases, there must be friction, and how this friction was utilised in the animal economy. He also described the powers and forms of the various muscles, and gave special attention to the most wonderful triangular muscle in the world, the biceps femoris muscle, or the flexor of the thigh of the tiger. While speaking of the strength of tigers, he narrated how he cut the claws of a tiger in the Zoological Gardens of Dublin, because the claws were growing into the body of the foot. Eight men held the tiger close to the bars of the cage by means of ropes. They kept one foot of the tiger off the ground, and this was a great point in the operation, because then the tiger was obliged to keep the other foot on the floor to keep from falling, so could not use it very offensively. As Dr. Haughton was on the point of cutting the claws, the tigress came up to see what was going on. She put out her paw, laid hold of Dr. Haughton by the hat, and in doing so, lifted up the door of the cage, leaving him face to face with the animals. The eight men resolved themselves into their component elements; all of them but one ran away. The tiger bent itself for a spring, Dr. Haughton pulled down the door of the cage, and the animal broke its teeth against the bars. The seven cowards were then collected, and the claws of the tiger were cut, after which the animal began to purr, and licked the hand of the operator.

The muscle in the leg of a man is like a rope of parallel prismatic fibres; in most other animals it is arranged as a prismatic muscle. He asked, "Why has nature deliberately sacrificed a certain amount of force by putting a triangular muscle into the leg of a tiger to do the work which she does so effectively in my leg by a straight rope or muscle?" The answer was that the weight of muscle necessary to enable the tiger to make his great springs was so enormous, that if it had been placed as a straight rope from point to point, it would not only be a great deformity in his appearance, but would seriously impede him in his progress through the jungle. "No, I cannot do that," says Nature, "I must preserve beauty of form," making the tiger the most beautiful creature which God has created. Therefore the tiger was given a triangular muscle, with a certain amount of loss of force; but there is a gain by spreading the muscle over a great surface—a gain in the packing and shape of the leg. The lecturer added that he was sorry to upset the superstitions of childhood, but the tiger is a much more powerful animal than the lion, and will always beat the latter in a fight. The lion had a great mane and looked big; still he was a humbug, like a good many people he knew.

Dr. Haughton said much about the wings of birds, and the muscles which govern their motions, after which he spoke of the force with which the muscles of the heart pump the blood through the veins of animals. He said that it had been proved in the early part of this century, that the muscular force

exerted by the living heart of a cow will raise a column of fluid blood nine feet high in a tube. The experiment cannot be made with the heart of a living man, as death would be the result, but the height to which the blood will spout when the artery of a man is cut is known; also the height to which the blood of a cow will spout under like conditions. By comparing the two phenomena, the hydrostatical pressure exerted by the muscles of the heart of a man has been approximately ascertained, as well as by another method. The blood of a horse or cow will spout about 2·53 feet high when an artery is cut, and the blood of a man 2·58 feet high; the blood would rise much higher than this in tubes, because, when an artery is cut, Nature makes an effort to close the orifice, to stop the bleeding. By various methods of experimenting he had come to the conclusion that the muscular force constantly exerted by the heart of a man is competent to raise between twenty and twenty-one pounds one foot high in a minute of time.

He also gave some little attention to the low musical note given by a muscle when it contracts. When a muscle contracts it gives a low sound, like the distant roar of cabs rattling along a rough street. There are several ways of hearing this sound. One is, when lying in bed at night, to lay one ear on the pillow, so that the pillow shall act as a sounding-board, and then to clench the teeth. The contraction of the muscle which acts upon the jaw, will then give the low roaring noise just mentioned. By means of tuning-forks, he and others had determined that this musical sound corresponded to 35½ double vibrations per second.

He said that when muscular fibre contracts it contracts to $\frac{3}{5}$ of its length. On measuring the muscular fibres of the heart, he found that they were so arranged that they could all contract to $\frac{3}{5}$ of their length, and some of them in so doing gave the interior of the heart a squeeze, which expressed the last drop of blood from the pumping apparatus, so that no force was lost. The human heart beats 57 times in a minute. He closed his lectures by speaking of the perfection of the muscle which gives birth to the young of animals, and with a few remarks to the effect that science was not opposed to theology.

CHEMICAL SCIENCE.

Further particulars have now reached us as to the remarkable discovery by Professor Seely of the solubility of metallic sodium and other metals in ammonia. He prepares the ammonia gas in an iron-generator, to which is attached a stout glass tube containing the metal to be subjected to the ammonia. When sodium is subjected in this apparatus to the condensing ammonia, before any ammonia is visibly condensed to the liquid state, it gradually loses its lustre, becomes of a dark hue, and increases in bulk. The solid then appears to become pasty, and at last there is only a homogeneous mobile liquid. During the liquefaction, and for a little time after, the mass is of a lustrous, copper-red hue; the condensation of the ammonia and its mingling with the liquid steadily goes on, the liquid is progressively diluted, and passing through a variety of tints by reflected light, at last it becomes plainly transparent and of a lively blue, as well by reflected as by transmitted light; the liquid now closely resembles a solution of aniline blue or other pure blue dye-stuff. On reversing the process by cooling the ammonia generator, the ammonia gradually evaporates out of the liquid, and the changes observed during the condensation re-appear in the reverse order, till at last the sodium is restored to its original bright metallic state. If the evaporation be conducted slowly and quietly, the sodium is left in crystals of the forms seen in snow. The formation of the transparent blue liquid and the restoration of the sodium are steadily progressive, and the repeated and closest scrutiny of the process has failed to reveal the slightest break or irregularity in its continuity. The inevitable conclusion from such facts is that the blue liquid is a simple solution of sodium in ammonia, not at all complicated or modified by any definite chemical action. The brilliant and varied colours exhibited in the experiment may seem anomalous to some, when, in fact, a closer scrutiny of the case will

show that they might have been predicted. Sodium appears white to the eye, but with the white light reflected from its surface to the eye there are always mingled red rays. If most of the incident white light were normally decomposed, sodium would appear as a brilliant red metal. Ammonia favours such a decomposition probably by reducing the density and opacity of the surface, and thus the concentrated solution of sodium is lustrous copper-red by reflected light. What should be the colour by transmitted light? Not red, for the red rays do not penetrate the substance; the colour must be looked for in that which is complementary to red; it must be blue or yellow, or combinations of these. A continuance of the argument will bring the conclusion that the colour by transmitted light will be blue. Intense blue tinctorial substances, like aniline blue, indigo, and Prussian blue, all illustrate the phenomena of colour of the sodium solution; they are metallic red when concentrated, and if the solvent be applied in vapour as in the sodium-dissolving experiment, there will be the same modification of colour exhibited. Sodium has a remarkable tinctorial power, which seems not to be surpassed by that of any of the aniline colours.

Referring to what was said in our January number, p. 127, about the earth-eating of the Javanese and the miners about Wurtemburg, a correspondent gives us the following interesting particulars:—He writes that an old gentleman, a friend of his, who was born and bred at Dunsire, near Dolphinton, in Lanarkshire, said that when a boy at school there, he and his schoolfellows used to eat a sort of clay which they found somewhere near the banks of the little river Medwin. He described it as of a light brown colour; it broke into thin plates about like thin oat-cake, was of a sweetish taste, and melted in the mouth. Perhaps it is yet to be found in that neighbourhood; and, if so, we should be very glad to have a sample for examination.

Gunning has discovered, in acetate of zinc, a reagent that precipitates the slightest traces of the colouring matter of blood from solutions, even where the liquids are so dilute as to be colourless. Blood, washed from the hands in a pail of water can readily be detected in this way. The flocculent precipitate, thrown down by acetate of zinc, must be washed by decantation, and finally collected on a watch-glass, and allowed to dry, when the microscope will readily reveal hæmin crystals if any blood be present.

The artificial preparation of coniine, an oily liquid, highly poisonous, and closely resembling the nicotine obtained from tobacco, has more than ordinary interest, as it suggests the possibility of our being able to make other alkaloids, such as quinine, morphine, and the like; and if we can succeed in this, why not prepare the less complex compounds, sugar, starch, &c.? Coniine has been artificially prepared by Hugo Schiff, by heating alcohol and ammonia at 210°, together with butyraldehyd, precipitating with a platinum salt, and distilling the product. The artificial alkaloid exhibits the same properties as the native, and is a violent poison. As the first step in the synthesis of vegetable alkaloids, the discovery of Professor Schiff is one of the most important in modern chemistry.

Recent occurrences have rendered anything connected with the consumption and production of food a matter of great interest. In a public lecture delivered at the Medical School at Paris, for the purpose of imparting some sound knowledge on the quantity of food required to keep man in a vigorous and healthy state, Dr. Sée said that the daily ration for an adult might be enumerated as follows:—100 grms. of meat, 20 grms. of salt fish, 750 grms. of bread, 50 grms. of lard, and 50 grms. of dried and compressed vegetables—a total of 970 grms. of solid food, containing 88 grms. of albuminous matter. Incidentally, the author observed that crust of bread contains just twice more nutrimental value than crumb, which contains 44 per cent of water. The highly nutritive value of wine was specially alluded to, and illustrated by the fact that, in some districts of France and Spain, men live on bread and wine only for many weeks together in a healthy and vigorous state.

Some experiments have been made with dogs on the influence which coffee and cacao exert as food, in which Dr. Rabuteau gave diets in one case

consisting daily of 20 grms. of bread, 10 grms. of fresh butter, and 10 grms. of sugar; in the other case, 20 grms. of cacao, 10 grms. of sugar, and an infusion of 20 grms. of well-roasted coffee. From these experiments the author draws conclusions leading him to consider coffee and cacao as simply preventing de-nutrition. This view has been objected to by MM. Payen, Dumas, and Chevreul. As regards cacao (commonly, but erroneously, in this country named cocoa), there can be no doubt that, containing as it does from 17 to 20 per cent of albuminous matter, with from 10 to 12 per cent of starch, from 40 to 50 per cent of fat, and among its mineral matter phosphates, it is food. M. Chevreul very properly observes, among other matters, the existence of idiosyncracy, and its influence on the individual tastes, and hence also more or less on the action of various alimentary substances, pointing out that he himself has, from his earliest years, an invincible repugnance against wine, milk, fish, and various vegetables, none of which he ever partakes of, but for all that it would, of course, be absurd to deny the nutritive properties and value of these substances.

The occurrence of the metal manganese in the mineral kingdom has been a matter of discussion and some little doubt. Dr. J. E. De Vry states that, while studying in Germany, thirty years ago, he collected beech-nuts in the vicinity of Giessen, and found the ashes of these nuts to contain a rather large percentage of manganese, which was readily explained by the fact of that mineral being present in that locality rather abundantly. In 1847, being at the meeting of the British Association at Oxford, the author gathered some unripe beech-nuts in Blenheim Park, which, on being afterwards tested, were found to contain a relatively large amount of manganese, although grown in a very different soil. A third analysis of the ashes of beech-nuts, collected in the wood of the Hague, confirmed the same fact.

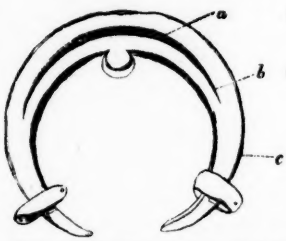
Dr. A. Vogel has described a simple experiment to illustrate the action of dilute sulphuric acid upon starch. Since nearly all kinds of writing paper are nowadays so very largely sized with starch that they are coloured blue by a dilute solution of iodine, the following experiment may be made to illustrate visibly the change which starch undergoes when acted upon by dilute sulphuric acid, for which purpose writing or other figures are traced on the paper with very dilute sulphuric acid, and a gentle heat applied, care being taken not to char the paper. When next a dilute solution of iodine is painted over this paper, the portion acted upon by the dilute acid remains white, while the rest becomes blue; but since, in course of time, this blue colour disappears altogether, the same piece of paper may be repeatedly used for this experiment.

The true constitution of artificial ultramarine has long been a matter of doubt. Dr. W. Stein has written a long essay on the mode of combination in which the sulphur is present in it. The author states that his aim is to prove that blue ultramarine contains sulphurous, not hyposulphurous acid, which former, however, is not an essential constituent; and further, that ultramarine contains only sulphuret of aluminium, there being no sulphuret of sodium at all. The conclusions drawn by the author from his experiments are—Ultramarine consists mainly of a white mass, through which and with which black sulphuret of aluminium is most intimately and molecularly incorporated; the blue colour of ultramarine is, therefore, not due to its chemical composition, but to the optical relation of its component substances. The essay winds up with some observations on white and green ultramarine; the latter contains less soda than the blue-coloured pigment, and that, again, less soda than the white pigment. The quantity of sulphur contained in blue-coloured ultramarine is less than that contained in green-coloured ultramarine.

The examination of some ancient silver ornaments has led Professor Church to a curious result, which may prove of interest to some of our readers. The silver objects referred to were obtained lately by General de Cesnola in the Island of Cyprus. He opened several hundreds of tombs, specially at Dali, the site of the ancient city of Idalium, and obtained large quantities

of specimens of antique glass and metal-work. The silver specimen with which the present note is concerned cannot have an antiquity of less than 1500 years, but the date of its manufacture may prove to have been even more remote. It was shaped like a fibula. It resembled a crescent in that the ends of the bow were small and pointed and its middle much stouter; sections throughout were nearly cylindrical, and in the widest part about ¼-inch in diameter. The specimen was nearly covered with a dark sub-metallic crust, and underneath this a soft powdery grey substance was observable. Both these layers, which together presented an average thickness of 1-30th of an inch, could be readily removed by the finger-nail. Below, the substance appeared white, metallic, and uniform, but yet was remarkable for its extreme brittleness, the object being easily snapped in numerous pieces by a very slight pressure. If the brittleness had pertained to the outer crust or layer, obviously altered as it was, it would not have been surprising. But to find that an apparently continuous metallic substance of great lustre, and, certainly once, at the time of its manufacture, in a perfectly ductile condition (for portions of it had been twisted into fillets), to find this metallic substance as brittle as a stick of chalk was certainly difficult to explain. And a further difficulty was offered by the observation that in the thicker part of the crescent, and only there, a kind of core existed similar in colour to the main mass of the object, but possessing great tenacity and ductility. This core could not be broken short off, but might be wrenched out of the surrounding brittle mass, though with some difficulty. It tapered off towards the narrower ends of the crescent, and terminated where those parts did not exceed ¼th of an inch in thickness. The outer crust consisted of the corroded and chemically altered silver-alloy. It was made up of finely-divided metallic silver, with traces of the sulphide and chloride of that metal, and it gave also a faint trace of iodide; copper, apparently in the form of a basic carbonate, was likewise present, with a trace of gold. The main part of the specimen consisted of the second and extremely brittle layer referred to before. On analysis it was found to have the composition :—Silver, 94·69 per cent; gold, 0·41; copper, 3·43; lead, 0·28; antimony, with trace of arsenic and bismuth, 1·21. The central ductile and malleable core had *precisely* the same composition as this brittle layer. To what, then, are the physical differences between them to be attributed? Not to chemical differences or chemical changes, but to a *molecular change* which, in the course of ages, has occurred throughout all the thinner parts of the piece of silver, but had left unchanged in the thickest part a central fusiform core. A crypto-crystalline structure had been produced in the previously homogeneous alloy, which caused the peculiarly

FIG. 8.



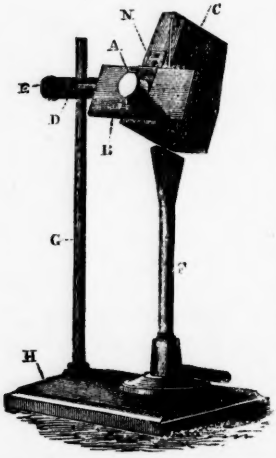
Section (in the plane of the paper) of the silver object referred to.

easy fracture of the metal. A smart blow with a hammer was capable of shattering it to powder, while by rolling or gentle hammering, the brittle mass could be restored readily to its pristine condition. The density of the brittle silver was 9·06, but, by rolling, it became 10·20. There was not, therefore, a chemical change, but a physical one. However produced, whether by minute

and long-continued alterations of temperature, or by other agencies, this molecular change of silver may possibly throw some light upon the similar change observed in the case of iron.

A very convenient arrangement for heating crucibles by gas is described by Professor J. Lawrence Smith, in Crookes's "Select Methods in Chemical Analysis," p. 408. It has been found of the greatest use in the analysis of silicates, but it will be equally useful in other branches of analysis where fusions at a high temperature are required. The figure here given illustrates the stand, burner, crucible, &c., and is about one-sixth the natural size. H is the stand with its rod, G. D is a brass clamp, with two holes at right angle to each other; having two binding screws, it slides on the rod, G; the second hole is for a round arm attached to B, the binding-screw, E, fixing it in any position. B is a plate of cast-iron, 5 to 6 m.m. thick, 10 to 11 centims. long, and 4½ centims. broad, having a hole in its centre large enough to admit the crucible to within about 15 m.m. of the cover without binding. A is the crucible, which is made to incline a few degrees downwards by turning the plate of iron that supports it. C is a chimney of sheet-iron, 8 to 9 centims. long, 10 centims. high, the width at the bottom being about 4 centims. at one end, and about 3 centims. at the other end. It is made with the sides straight for about 4 centims., then inclines towards the top, so as to leave the width of the opening at the top about 1 centim. A piece is cut out of the front of the chimney of the width of the diameter of the hole in the iron support, and about 4 centims. in length, being semicircular at the top, fitting over the platinum crucible. Just above this part of the chimney is rivetted a piece of sheet iron in the form of a flattened hook, N, which holds the chimney in place by being slipped over the top of the crucible support; it serves as a protection to the crucible against the cooling effects of the currents of air. F is the burner, the upper opening of which is a slit from 1½ to 2 m.m. in width, and from 3 to 4 centims. long, and when used is brought within about 2 centims. of the lowest point of the crucible, the end of the flame just playing around the lower end of the crucible. The gas enters the lower part of the burner by two small holes of 1-16th of an inch, furnishing at 1-inch pressure about 5½ cubic feet of gas per hour. It is surprising to see the effect produced by this simple burner as here used; 8 grms. of precipitated carbonate of lime can be decomposed to within 2 or 3 per cent in one hour, and when mixed with silica or a silicate, in a very much shorter space of time. A method invented by Thiercelin has been successfully used in extracting iodine from crude Chili saltpetre. The mother-liquors resulting from the manufacture of saltpetre are treated with a mixture of sulphurous acid and sulphite of soda, and the iodine will be precipitated as a black powder. This is placed in earthen jars, on the bottom of which are layers of quartz-sand, fine at the top and coarse at the bottom; from these jars it is removed by earthen spoons lined with gypsum, and the greater part of the water is thus separated. It is further purified by sublimation, but is often sold before undergoing the last-named process. The amount of iodine thus reclaimed from Chili saltpetre already amounts to 30,000 pounds per annum.

FIG. 9.



Up to a few years ago indigo was considered to be insoluble in almost all agents which did not decompose it. Now several agents are known

in which it can be dissolved and from which it crystallises on cooling. So far back as 1861 an editorial article in the "Chemical News" contains the following passage:—"Hot aniline dissolves indigo, forming a solution having almost exactly the tint of the solution of azuline in methylated spirit. On cooling, the indigo crystallises out in beautiful coppery spangles. This method of crystallising indigo from a solution in boiling aniline does not appear to be generally known to chemists." Dr. V. Wartha now states that Venetian turpentine, heated to its incipient boiling-point, is a solvent for indigo, which, after cooling, is readily purified by the aid of ether or alcohol. Boiling paraffin also dissolves indigo-blue, and the paraffin can be removed, after cooling, by means of benzol. Spermaceti, stearic acid, and chloroform, all at a high temperature, are stated to be more or less good solvents for indigo.

According to the German chemist Kopp, a mixture of two liquids boils at a lower temperature than either of the component liquids separately. Mr. D. Howard has verified this law in the case of a mixture of amylic alcohol and water, which boils at a lower degree than pure water.

M. A. Valenciennes, of the chemical works at St. Denis, near Paris, has lately prepared metallic manganese and several of its alloys. The metal was obtained by the reduction of pure binoxide of manganese in a magnesia crucible, and formed a brittle and very hard button. Immediately after breaking it, the pieces were as white as cast-iron, but more rapidly oxidised by the air. Manganese shows great affinity for copper. Valenciennes prepared alloys of copper and manganese, containing from 3 to 20 per cent of manganese, all of which resemble very much the copper-tin alloys (bronzes), being, like these, hard, sonorous, and easily fusible. The alloy, containing 15 per cent of manganese, is grey, very hard, brittle, fuses like bronze, is easily cast into moulds, and remains unchanged for some time. The alloy with 12 per cent of manganese is also brittle and very hard, grey after being turned, but soon becomes yellow as brass.

A cold solution of bichromate of potassa in nitric acid is, according to Dr. Böttger, an excellent test for the genuineness of silver plating on metals. The metallic surface to be tested is first of all cleaned with strong alcohol, in order to remove dirt, fatty matter, and especially any varnish. A drop of the test fluid is then applied to the metallic surface by means of a glass rod, and immediately afterwards washed off with some cold water. If pure silver is present, there will appear a blood-red coloured mark (chromate of silver). Upon German silver the test liquid appears brown, but after washing with water the blood-red coloured mark does not appear; the so-called Britannia-metal is coloured black; on platinum no action is visible; metallic surfaces coated with an amalgam of mercury yield a reddish speck, which, however, is entirely washed off by water; on lead and bismuth the test liquid forms a yellow-coloured precipitate; zinc and tin are both strongly acted upon by this test liquid; the stain as regards the former metal is entirely removed by water, while, as regards the latter, the test liquid is coloured brownish, and addition of water produces a yellow precipitate which somewhat adheres to the tin.

A method of imparting a yellowish hue to white marble is sometimes wanted. Dr. R. Weber has made known the fact that alcoholic solutions of perchoride of iron are not precipitated by carbonate of lime, and may therefore be applied in different degrees of concentration to impart a more or less deep yellow hue to white marble. The converse of this—the removal of yellow stains from white marble—is still a desideratum.

In experimenting upon the mother-liquors of the manufacture of sulphate of quinine, Mr. D. Howard has occasionally been perplexed by an unusual loss in re-crystallising, which the mechanically adhering mother-liquor did not seem to account for. A more careful examination showed that the cause was the presence of an alkaloid hitherto undescribed, the extreme solubility of the salts of which both distinguishes it at once from the chinchona alkaloids already known, and renders it very difficult to separate from the uncrystal-

lisable quinoidin. The platino-chloride is almost insoluble in water or in cold hydrochloric acid; it forms a crystalline powder by precipitation, and well-defined crystals by solution in acid. It is isomeric with the platino-chloride of quinine, but anhydrous. The author has investigated the sulphate, tartrate, citrate, hydrochlorate, phosphate, and sulphocyanide of the new base. He has, however, not yet found out whether this alkaloid is contained in all the species of cinchona, or, if not, in which of them.

Dr. H. Fleck has published the account of a series of experiments made with the view to ascertain how far it is possible to substitute for the ordinary process of malting the method of steeping the grain (barley or any other) in weak acids, to obtain thereby the same effect as produced by germination, but in a far shorter period of time. It appears that, provisionally, the author has succeeded in his attempt, but is engaged in further experiments. Dilute nitric acid, containing 1 per cent of acid, yields excellent results.

It is well known that, when glycerine, subjected to the ordinary atmospheric pressure, is heated so much as to cause ebullition, it is more or less rapidly decomposed by repeated distillations. This decomposition may be, however, entirely prevented by a reduction of the pressure in the apparatus employed to 12·50 m.m. Mr. T. Bolas, who has worked for some time on this subject, has determined the boiling-point of glycerine, by effecting the distillation in a long-necked flask, having a supplementary neck attached at right angles to the principal one. In the principal neck the thermometer was fixed by the aid of a caoutchouc cork, while the smaller neck was connected in a similar manner with a two-necked receiver. The glycerine, together with a few fragments of tobacco-pipe (this latter being required to prevent the bumping which would otherwise occur), being placed in the retort-flask, the receiver was connected with a Sprengel's mercurial pump and a manometer, the caoutchouc joints being made air-tight with glycerine in the usual way. Unless the glycerine distilled had been dehydrated by previous distillation in a vacuum, the first portion of the distillate consisted principally of water; afterwards, when the glycerine in a pure state came over, the temperature indicated by the thermometer was 179·5° C., the pressure on the liquid being 12·5 m.m. Under a pressure of 50 m.m., glycerine distils without change at about 210° C. Glycerine, dehydrated by distillation, absorbs water from the atmosphere to the extent of about 50 per cent of its weight.

It is a striking lecture experiment in illustration of the fact that the terms combustible and supporter of combustion are only conventional, to show that in an atmosphere of hydrogen, oxygen burns with a flame, just as hydrogen does in air or oxygen. Professor Thomsen, of Copenhagen, has refined upon this experiment by making oxygen burn with a sooty flame. Take a long-necked flask; pour into it some benzol or oil of turpentine; close the flask with a doubly-perforated cork through which two short glass tubes are passed, one of which should be of about 1 centimetre internal diameter, the other narrower and somewhat bent sidewards. Let the liquid in the flask be boiled, and, as soon as the vapours issue from the wider tube, ignite them; and, this having been done, there is passed through that tube another narrower glass tube connected with a suitable gas-holder containing oxygen. This tube should be provided with a platinum burner bent upwards and fitted with a cork, which closes the opening of the wider tube. This oxygen-carrying tube is made to pass deep into the flask; and immediately after the closing of the 1 centimetre wide tube, the oxygen begins to burn with a sooty flame, while the excess of the vapours of the boiling liquid are discharged by the narrower glass tube above alluded to.

Dr. E. von Gorup-Besanez has given an account of the very serious effects of the explosion of only 10 drops of nitroglycerine, which were put into a small cast-iron saucepan by one of the pupils of the author, in his laboratory, and heated with a Bunsen gas-flame. The effect of the explosion was that forty-six panes of glass of the windows of the laboratory were smashed to atoms, the saucepan was hurled through a brick wall, the stout iron stand on which the vessel had been placed was partly split, partly spirally twisted, and the tube

of the Bunsen burner was split and flattened outwards. Fortunately, none of the three persons present in the laboratory at the time were hurt. The author takes the opportunity of referring to a communication of Dr. E. Kopp, bearing upon the conditions under which nitroglycerine explodes or burns off quietly. When nitroglycerine is caused to fall drop by drop on a thoroughly red-hot iron plate, it burns off as gunpowder would do under the same conditions; but if the iron is not red-hot, but yet hot enough to cause the nitroglycerine to boil suddenly, an explosion takes place.

Dr. Von Kobell gives an account of the testing of various minerals containing lithia with the spectroscope. The author puts the pulverised minerals on a piece of platinum foil formed like a channel, perforated with small holes, and held by means of a pair of platinum forceps. The author observes that, unless the mineral to be tested for lithia is previously well moistened with hydrochloric acid, the detection of lithia may fail, since only the spectrum of the flame tinged by chloride of lithium exhibits the characteristic line. Several minerals have also been tested by this author for thallium, and more especially some zinc ores. Most of these minerals gave a negative result; but the sphalerite (a kind of zinc blende) from Geroldseck, and a similar mineral from Herbesthal (a small Prussian village close to the Belgian frontier), gave very evident indications of the presence of thallium.

The popular toy, "Pharaoh's Serpents," has gone almost out of use, owing, probably, to the poisonous nature of the ingredients and the danger of inhaling the mercurial fumes they evolved. Dr. Puscher has published the recipe for a mixture for producing so-called Pharaoh's Serpents which are not attended with injurious fumes. Mix intimately together 2 parts of bichromate of potassa, 1 part of nitrate of potassa, and 3 parts of white sugar. This mixture should be pressed in paper or tinfoil cones, and, if intended to be kept for any length of time, the paper should be varnished over with sandarac varnish. A small quantity of balsam of Peru may be added to perfume the mixture, so as to cause its combustion to be attended with a pleasant odour. The greenish coloured very porous mass, which assumes the serpent shape, is a mixture of carbonate of potassa, oxide of chromium, carbon, and a small quantity of neutral chromate of potassa. The author states that this mass is an excellent substance for polishing all kinds of metals. The mixture above specified should be kept in a dark place.

Dr. Gräger has proposed a new method for the reduction of chloride of silver dissolved in ammonia by means of metallic zinc. This process, according to the author, succeeds very well, and yields a silver of greater purity than is obtained by the process for reduction of silver by the moist way now in use, provided the operation be carried on in closed bottles. The silver, after complete reduction, is first thoroughly washed with concentrated hydrochloric acid, next with water, and, lastly, for some moments with dilute ammonia, and then again with distilled water. It is clear that this method of reduction involves the use of a considerable quantity of ammonia; but this, the author states, can in great measure be recovered by distillation.

A mode of distinguishing the deposit of arsenic obtained by Reinsch's process from salts of mercury has been published by Mr. James St. Clair Gray. It is for mercury a test of even greater delicacy than that afforded by sublimation of metallic globules from the coated copper foil, and is possessed also of the advantage that the result can be seen by the unaided vision, while its application is extremely simple. It is founded on the great affinity which exists between mercury and gold. One of the copper slips, coated in the ordinary manner by Reinsch's process, is first washed in pure distilled water and then thoroughly dried; when thus prepared it is rubbed with a flattened bead of pure gold, or, should this not be at hand, a flat signet ring will suffice. The result, if the coating be mercurial, is that a portion of the mercury, whose affinity for gold is greater than for copper, is transferred from the copper to the gold, appearing on its surface as a clear, white, shining, metallic crust, this being more conspicuous the more highly coloured the surrounding gold is. The stain is at once removed by pure strong nitric acid. This is of itself perfectly conclusive

of the presence of mercury in the metallic coating or deposit on the copper, and is equally applicable should there exist in that deposit a combination of mercury with any or all of the other three metals which yield by Reinsch's process a metallic deposit on copper foil.

MINERALOGY.

At present we are in great measure ignorant of the conditions which are necessary for determining the appearance of certain faces in a crystal. Chemical research has, indeed, afforded some insight into this subject; we know, for instance, that alum may be made to crystallise in cubes by the presence of alkaline carbonates; in octahedra by the presence of sodic nitrate; and in a combination of the two forms by the presence of cupric nitrate. In the case of most native crystals, however, the subject is still so obscure that any attempt at its elucidation must needs be welcome. Dr. A. Stelzner, of Freiberg, who has, we believe, been recently appointed Professor of Mineralogy in the University of New Cordova, publishes a memoir, in which he seeks to discover what natural causes have induced the presence of trapezohedral planes in certain crystals of quartz.* Compared with the ubiquity of quartz the appearance of these faces is extremely rare. The author collects all the known instances of their occurrence, and records the accompanying minerals, with their relative age. These paragenetic studies show that the minerals associated with trapezohedral quartz are chiefly apatite, axinite, datholite, fluor-spar, mica, topaz, tourmaline, beryl, scheelite, iron-glance, anatase, rutile, brookite, sphene, wolfram, and cassiterite; and that the quartz must in general have crystallised nearly contemporaneously with these substances. As many of these associated minerals contain compounds of fluorine, chlorine, or boron, the author deduces the conclusion that the presence of these elements must be connected with the trapezohedral habit of quartz. Moreover, it is found that small faces of this form may be developed on quartz by etching the surface with hydrofluoric acid. The author's studies lead him to the general conclusion that the trapezohedral form of quartz is restricted to those cases in which the crystals have been formed contemporaneously with certain minerals of the tin or titanium formation, with development of hydrochloric or hydrofluoric acid.

Mr. Robert H. Scott, of the Meteorological Office, has presented to the Geological Society some notes on the interesting group of minerals found in the mines of Strontian, in Argyllshire. This locality, which gives its name to the element *Strontium*, contains several lead-mines, which yield many rare zeolitic minerals, especially harmotome and Brewsterite. The former has been found chiefly at Bell's Grove Mine, and the latter at Middle Shap Mine, whilst both of them have been obtained from Whitesmill Mine. At the newly-opened workings at Corrantree, fine crystals of calcite occur in association with well-twinned crystals of harmotome. The mine is opened in gneiss, whilst the old mines were worked at the junction of the gneiss with the granite. In all these workings the gangue is remarkable for the absence of fluor-spar, and the comparative rarity of zinc-blende and barytes; whilst the galena is but slightly argentiferous.

Eosite is the name which Dr. Schrauf has applied to a new mineral from Leadhills, in Scotland. It occurs in small aurora-red octahedra, sparsely scattered over the surface of a greenish-yellow cerussite, and accompanied by very small yellow needles of pyromorphite. The new species crystallises in the pyramidal system, and contains molybdenum, vanadium, and lead, forming a connecting-link between wulfenite and vanadinite, and yet essentially different from the well-known forms of red wulfenite. The name has reference, we presume, to the aurora-red colour of the crystals.

A new mineral discovered by the distinguished geologist, Dr. Gümbel, has very properly received the name of *Gümbelite*. It is a hydrous silicate of

* Quarz und Trapezoëderflächen: eine paragenetische Skizze. Leonhard u. Geinitz's Neues Jahrbuch, Heft i., 1871, p. 33.

alumina, potash, ferric oxide, &c., and occurs at Nordhaben, near Steben, in Upper Franconia. The mineral forms thin shortly-fibrous layers on clay-slate and on iron pyrites, and presents a greenish-white colour, coupled with a pearly lustre.

The well-known locality, Lüneburg, in Hanover, has given its name to a new species termed *Lüneburgite*. It is a boro-phosphate of magnesia, probably deposited from the mother-liquor of sea-water.

Under the name of *metacinnabarite*, a native amorphous sulphide of mercury has been described by Mr. G. E. Moore. The mineral was found by Professor Whitney in Lake Co., California, and contains—sulphur, 13·82; mercury, 85·79.

According to the Abbé Moigno, a valuable deposit of native phosphate of lime has been discovered near St. Petersburg. The layer occurs at a depth of about 1·8 metre below the surface, and was brought to light during some recent excavations for drainage.

Von Kobell has detected thallium in the zinc-blende, or sphalerite, of Geroldseck and Herbesthal; whilst the blende of many other localities was examined for this element with only negative results.

It is so long since Dr. Schrauf issued the first part of his fine "Atlas of Crystal-Forms," that we had almost ceased to expect a continuation of this valuable work. It appears, however, that the delay has arisen from causes which neither author nor publisher could control, and we may now hope that the succeeding parts will follow in rapid succession. The second instalment just issued* contains clear drawings of crystals of anglesite, anhydrite, anorthite, antimony, antimonite, apatite, and aphanesite. It may be well to note that this last name is applied to the arseniate of copper, so well known as clinoclase—a name which, in spite of its priority, has been transferred from this species, and applied to some of the felspars—clinoclase being of course the proper correlative of orthoclase and plagioclase.

Many attempts have from time to time been made to represent mineral specimens by plates painted in natural colours, but such attempts have rarely been successful. A second edition of Weber's work "On the Minerals of Bavaria," has now appeared under a new title,† and contains 64 plates of minerals tinted after nature. The present edition has been prepared with the assistance of Dr. Haushofer.

A work on the mineralogy of volcanoes, by Dr. Landgrebe,‡ gives in alphabetical order descriptions of all minerals found in rocks of volcanic origin, whether these minerals are original constituents of the rocks or merely subsequent products. Its especial value lies in notices of minerals occurring in the two Hesses and the adjacent countries, where the author has done original work. For example, he has discovered graphite in the columnar basalt of the Lammsberg.

Naumann's "Elements of Mineralogy" has now reached an *eighth* edition.§ It is only two years since the last edition was issued, but the form of the work has since been much improved, and formulæ are now given on the modern atomic weights.

TECHNOLOGY.

The first report of the Royal Commission on Scientific Instruction and the Advancement of Science has just been published. From it we make the following abstract:—The commissioners consider that there is no necessary con-

* Atlas der Krystall-Formen des Mineralreiches. Zweite Lieferung. Tf. xi.—xx. Wien, 1871.

† Die Mineralien in 64 coloriten Abbildungen nach der Natur, von J. C. Weber. Zweite Auflage: verbessert und vermehrt unter Mitwurkung von Dr. K. Haushofer. 8vo. München. Pp. 99.

‡ Mineralogie der Vulcane, von G. Landgrebe. 8vo. Cassel und Leipzig. Pp. 396.

§ Element der Mineralogie, von C. Naumann. Achte vermehrte und verbesserte Auflage. Mit 836 figuren. 8vo. Leipzig. Pp. 606.

nection between the direction of the Geological Survey of Great Britain and Ireland and the government of the Royal School of Mines. The Royal School of Mines and the Royal College of Chemistry, which practically constitute one school of pure and applied science, are not organised in such a manner as to enable them to perform efficiently the work for which they were originally, or are, at present, intended. This conclusion is based upon three grounds: (*a*) the absence of a chair of mathematics; (*b*) the absence of physical or biological laboratories in which students can receive practical instruction; (*c*) the insufficiency of accommodation in the Royal College of Chemistry. After enumerating the various inconveniences attending the present arrangement, the commissioners recommend: (*a*) That the building in Jermyn Street be given to the Survey and to the Museum, with the reservation that the lectures to working men be delivered as heretofore in the theatre; (*b*) That the building in Oxford Street be vacated by the Royal College of Chemistry; and (*c*) That the Mining Record Office be lodged with the statistical department of the Board of Trade, or, failing accommodation there, in the building now occupied by the Royal College of Chemistry. The commissioners also recommend that the Royal School of Mines and the Royal College of Chemistry be consolidated; that mathematics be added to the courses of instruction now given; and that sufficient laboratories and assistance for giving practical instruction in physics, chemistry, and biology be provided. The institution thus formed (hereinafter called the "Science School") may be conveniently and efficiently governed by a council of professors, one of that body acting as dean. Attention is drawn to the buildings at South Kensington, now nearly completed, and intended for the reception of a projected school of naval architecture and science; and a recommendation is made that the science school should be accommodated in these buildings. Concerning the Royal School of Naval Architecture and Marine Engineering, now conducted at South Kensington, they recommend that the theoretical instruction of that school should in future be given in the science school, the general instruction in mathematics, physical science, and mechanical drawing thus becoming common to both schools. They also recommend that no additional buildings, and no reconstruction of the temporary buildings at present occupied by the Royal School of Naval Architecture and Marine Engineering, should be undertaken until a further report has been received from this commission. Referring to the system of teaching elementary science under the science and art department, the commissioners are of opinion that the quality of the instruction given under this department would be greatly improved if the teachers received practical instruction in elementary science. Such instruction has, indeed, already been given with marked advantage, although only to a limited extent. The science school will be available for the instruction of many science teachers throughout the country.

From a lengthy series of researches by Dr. A. Schwarzer on the conversion of starch into sugar by the action of the diastase of malt, it appears that—(1) the conversion of starch is quickened by an increased quantity of diastase, as well as a higher temperature; (2) that when starch fails to be detected by iodine, the formation of sugar is nearly finished; (3) at all temperatures from 60° C. to 0° downwards, and the application of varying quantities of diastase, there are formed always from 50 to 53 per cent of sugar; (4) at temperatures above 60° less sugar is formed than at temperatures below that degree; (5) a temperature of 70° impairs, and in some instances destroys, the activity of diastase.

Vulcanised rubber has been long an object of study and experiment, to see what use could be made of the waste pieces and cuttings; after the sulphur had been added, it was thought that it could not be worked over again, and this being the case, the price was likely to remain high for many years. Fortunately, the difficulty has yielded to the researches of practical men, and a process has been discovered by which the old rubber can be mixed with the fresh in certain proportions, and thus changed to a useful article.

Dr. Gräger has proposed a new method for the regeneration of waste nitrate of silver solutions used in photography. After first referring to the generally

applied and well-known means now in use for this purpose, the author states that the best plan to treat these solutions is the following:—They are boiled either in a porcelain basin or glass flask, and while boiling there is added to them recently precipitated, well washed, and moist oxide of silver, the boiling being continued for some time. The liquid is next filtered, and then evaporated to dryness, the heat being increased to fusion, so as to destroy ammoniacal salts; the residue is pure nitrate of silver. The sediment on the filter contains some oxide of silver, which must be added in excess; and, therefore, in order not to lose that, the filter is preserved, and the contents worked up at a subsequent operation. The nitrate of silver thus obtained is, by practical photographers, pronounced to be of excellent quality.

For coating brass with copper, Dr. C. Puscher dissolves 10 parts, by weight, of sulphate of copper, and 5 of sal-ammoniac, in 150 parts, by weight, of water. Place the brass, well cleaned and free from grease, into this mixture, leave it in it for a minute, let the excess of liquid drain off first, and heat the metal next over a charcoal fire, until the evolution of ammoniacal vapours ceases and the coppery film appears perfect. Wash with cold water, and dry. The coating of copper adheres firmly.

The best varnish to protect polished metals from rusting is, according to Dr. C. Puscher, a solution of paraffin (1 part, by weight, in 3 parts of petroleum). This varnish may be usefully applied to polished metals, especially as, after having brushed the liquid over the surface of the metals, they may be gently wiped clean with a soft piece of flannel, so as to leave only a very thin film of the varnish, yet sufficient for the protection of the polish.

The protection of iron water-pipes from oxidation is very necessary, not only to provide against the premature giving way of the metal, but to secure the scarcely less important *desiderata* of avoiding any undue roughening of the internal surface or the diminution of the bore by the accumulation of rust. The most successful means of preventing the results just referred to consists in coating the surfaces with a tenacious varnish. That used on the pipes of the Cochituate Water-works in Boston has withstood a ten years' test, and at the end of that time the pipes "were found to be almost free from rust or ochreous deposits." It was composed of coal-tar, distilled until the naphtha was wholly eliminated, and the tar or pitch brought down to the consistency of wax. The tar reduced to this condition was heated to 300°, a temperature at which it was kept during the dipping operation, and about 8 per cent of linseed oil added to it. The pipes of cast-iron were heated to a degree determined by the judgment of the engineer in charge, and immersed in the prepared tar for from thirty to forty-five minutes, at the expiration of which they were placed in such position to drip that the remaining layer or coating of the material should be of uniform thickness. This method of protecting the pipes is now exclusively adopted in the water-works of most of the cities in the United States, and its advantages have been found to be far in excess of its cost.

For the purpose of obtaining decorative colours on metals, Dr. Puscher employs a solution of hyposulphite of lead in hyposulphite of soda, in the proportion of 3 parts, by weight, of hyposulphite to 1 part of acetate of lead. The clear solution of the salts deposits, when heated to about 100°, a thin layer of sulphide of lead upon any metal—brass, zinc, copper—placed in the solution, and thereby produces a beautiful display of various colours on these objects.

The native Indians of Mexico employ a rapid method of tanning hides and skins, which may prove useful as a basis for experiment in more civilised countries. It appears that they form, with the brains of the animal and certain vegetable products locally called *cascalote* and *huizache* (substances containing a large proportion of tannic acid), an emulsion, which is forced into the skins by means of squeezing between the hands, or, more rapidly, by stamping with the bare feet. In some parts of Mexico, 4000 or 6000 goats are killed at

one time, simply for the skin and for boiling down for fat; the blood, flesh, bones, and offal simply being applied as manure.

Many attempts have been made of late years to manufacture artificial stone, some of which have been attended with more or less success, as far as regards the production of large blocks, but have rarely succeeded for the purpose of moulding or making medallions and mosaics, or for emery-wheels, oil-stones, and articles of a similar character. In almost all cases, too, the agency of heat has been required for producing articles in artificial stone, and this has been found very objectionable, as well as expensive. Attention has recently been called to some specimens of artificial stone in the shape of medallions, mosaics, emery-stones, oil-stones, imitation marble, &c., which have been produced in a simple manner, by moulding without the agency of heat, and which appear to partake in every respect of the nature of the stones they are intended to represent. These specimens are formed from natural materials, agglomerated by means of a cement, the invention of M. Sorel, a French chemist, well known in connection with the introduction into this country of the galvanised iron process. This new cement is formed of basic oxychloride of magnesium (oxide of magnesium and chloride of magnesium), either pure or mixed with other substances. The cement may be made in two different ways, viz., either by diluting or tempering magnesia, which may be more or less hydrated and carbonated, with a solution of chloride of magnesium more or less concentrated; or by adding to the magnesia chloride of magnesium in a dry state, and employing water to form the cement. The cement thus produced is specially white and hard, and may with advantage be used in place of some of the best cements. It possesses the same hardness, and will receive the same polish as marble, thus enabling it to be employed in the manufacture of artificial marble, mosaic pavements, and statuary. Imitation ivory can be made from it for making billiard balls and other similar articles, also medallions, buttons, &c. By combination with mineral colours, the cement may be made to assume any desired tint, may be moulded like plaster, and be employed in the manufacture or imitation of innumerable objects of art or ornament. In practice, the cement is never used used in a pure form, but in combination with other materials; which, being incorporated with it while in the moist condition, are in the subsequent setting mechanically bound together into a solid mass. For this purpose, the magnesia, in fine powder, is mixed with mineral substances, such as sand, gravel, dust, or chips from marble or other stones, or with emery, quartz, or other grits of various kinds, in varying proportions, according to the result desired. This mixture is then moistened with a solution of the chloride of magnesium, or with the bittern from salt works. In some cases it is made sufficiently wet to form a mortar, and in others only to produce a condition of dampness like that of moulding sand. The mixture may be effected in troughs, hy hand labour, the materials being worked over with shovels or hoes; or, more expeditiously, in mixing machines designed expressly for the purpose, and worked by horse or steam power. The materials of which this cementing substance is composed are abundantly distributed over the surface of the globe. Magnesia sufficiently pure for the purpose is obtained by simply calcining mineral magnesite, large deposits of which are known to exist in Prussia, Greece, Canada, California, Pennsylvania, and Maryland. Deposits will doubtless be found in other places when the demand is made for the material. The chloride of magnesium is readily obtained by concentrating sea water, the bittern of salt works being sufficiently pure for the purpose. Sea water concentrated at 30° B. precipitates nearly the whole of its chloride of sodium. In the "Sorel" process, it is concentrated to 33° B., when all the chloride of sodium is practically crystallised and precipitated, the mother-liquor retaining, besides the chloride of magnesium, some chloride of potassium and some sulphate of magnesia, which seem to add strength to the cement, as the water in this state makes a stronger stone than the pure chloride of magnesium.

LIGHT.

At the recent *soirée* of the Royal Society, Mr. J. Browning exhibited a spectroscope, in which the rays of light pass through two batteries of prisms, and were then sent back through both trains of prisms, by means of a reflecting prism placed behind the last prisms of the train, to the eye of the observer. In this manner a dispersive power equal to nineteen prisms of flint-glass is obtained. Both trains of prisms, as well as the intermediate prism, are under the control of an automatic movement, which ensures that every ray shall pass at the minimum angle of deviation, for the particular ray under examination. Both the collimator and telescope are fixed; the prisms only are movable. Mr. Proctor suggested this extension of Mr. Browning's automatic spectroscope, and contrived the form of prisms which would enable the automatic action to be carried on, but the mechanical difficulties, in communicating the automatic motion to the second battery were very considerable. These difficulties Mr. Browning has ingeniously surmounted. The D lines in the spectrum of sodium are seen in this instrument, separated by an apparent interval of more than $\frac{1}{4}$ of an inch, and under favourable conditions of the atmosphere, ten or twelve lines are visible between them. We understand that Mr. Spottiswoode, the Treasurer of the Royal Society, has become the possessor of this instrument.

The subject of signalling communications between fortresses and distant armies has occupied the attention of our French neighbours for a considerable time. M. Le Verrier has recently drawn attention to a lengthy paper containing the record of a series of experiments made for the Chief Committee of Defence of the Valley of the Rhône, Lyon, for the purpose of communicating, by luminous signals, at distances of 30 to 100 kilometres. It appears that, under the guidance of the well-known *savant* just named, excellent results have been obtained, and so simple in execution that ordinary workmen can satisfactorily manage the signals. The considerable expenses of these researches have been defrayed by a private individual, M. Maistre, of Villeneuvette, near Clermont, Hérault.

M. Delaurier, of the French Academy of Science, has taken up the proposal of M. Felix Lucas, a Government engineer, to obtain a very powerful electric light for coast service, at a comparatively small cost, by making the light intermittent, with intervals of two seconds; by this means, it is said, one 10,000th part of the electricity expended for a constant light will suffice. M. Lucas proposed to use static, but M. Delaurier prefers dynamic, and especially induced electricity, and suggests the following arrangement:—A battery which is only in action when the circuit is open, the couples united together in series, and the electricity passed through coils of solid iron wire; a very long copper wire, covered with silk, is wound around the iron wire-coil, the copper being sufficiently thick not to be heated by the passage of the current. Each pole is provided with a charcoal point; when these are in contact the current passes, and when they are separated the sparks are excessively brilliant, because both the direct and the extra current are brought into action, resulting from the induction of the current on itself and that by the coil of iron wire. The charcoal points are favourable to a spark of short duration. A very simple arrangement of clockwork causes the points to come into contact every two seconds, and then separates them sharply, so as to break the current instantaneously, It is not possible to obtain by these means a flash of such short duration as with static electricity, because the current passes for an instant through the air when the charcoal points are separated. It is believed that this system will be found useful, not only for piercing fogs at sea, but also for railway signals.

C. Schultz-Sellack has communicated a series of experiments with the object of proving that, while the chloride, bromide, and iodide of silver are acted upon by light, and altered in a well-known manner, this action is due to a phenomenon of dissociation, called forth by the light; but when these salts are put in sealed tubes, and care taken to have an excess of the haloids

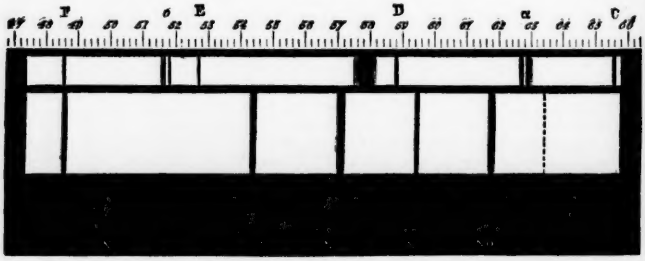
XUM

in free state present, and the tubes exposed to light, a mechanical change is effected, since the previously crystalline salts—obtained in that state from the ammoniacal solutions of the chloride and bromide and hydriodic acid solution of the iodide of silver—become at first dull, and next crumble to powder. This mechanical change of the haloid salts of silver is greatest when the chemical change is smallest.

The change produced in animal and vegetable forms by the influence of varying conditions of temperature, moisture, light, locality, &c., has induced many persons to try experiments on the influence of colour on insects, from which some interesting results have been derived. In some of these experiments, lately published, a brood of caterpillars of the tortoise-shell butterfly was divided into three lots. One-third were placed in a photographic room lighted through orange-coloured glass, one-third in a room lighted through blue glass, and the remainder kept in an ordinary cage in natural light. All were fed with their proper food, and the third lot developed into butterflies in the usual time. Those in the blue light were not healthy, a large number dying before changing; those raised in the orange light, however, were nearly as healthy as those first mentioned. The perfect insect reared in the blue light differed from the average form in being much smaller, the orange-brown colours lighter, and the yellow and orange running into each other instead of remaining distinct. Those raised in the yellow light were also smaller, but the orange-brown was replaced by salmon-colour; and the blue edges of the wings seen in the ordinary form were of a dull slate. If changes so great as these can be produced in the course of a single experiment, it is probable that a continuance of the same upon a succession of individuals would develop some striking results.

By means of the equatorial refractor of 15 inches aperture, by Messrs. Grubb and Son, recently placed in Dr. Huggins's hands by the Royal Society, this eminent physicist has succeeded in making observations described below of the remarkable spectrum which is afforded by the light of the planet Uranus. The spectrum of Uranus, as it appears in this instrument, is represented in the accompanying diagram. The narrow

FIG. 10.



spectrum placed above that of Uranus gives the relative positions of the principal solar lines, and of the two strongest absorption-bands produced by our atmosphere, namely, the group of lines a little more refrangible than D, and the group which occurs about midway from C to D. The scale placed above gives wave-lengths in millionths of a millimetre. The spectrum of Uranus is continuous, without any part being wanting, as far as the feebleness of its light permits it to be traced, which is from about C to about G. On account of the small amount of light received from this planet, a slit sufficiently narrow to bring out the Fraünhofer lines could not be used. The positions of the bands produced by planetary absorption, which are broad and strong in comparison with the solar lines, were determined by the micrometer

and by direct comparison with the spectra of terrestrial substances. The spectroscope was furnished with one prism of dense flint-glass, having a refracting-angle of 60°, an observing telescope magnifying 5½ diameters, and a collimator of 5 inches focal length. A cylindrical lens was used to increase the breadth of the spectrum. The remarkable absorption taking place at Uranus shows itself in six strong lines, which are drawn in the diagram. The least refrangible of these lines occurs in a faint part of the spectrum, and could not be measured. Its position was estimated only, and on this account it is represented in the diagram by a dotted line. The positions of the other lines were obtained by micrometrical measures on different nights. The strongest of the lines is that which has a wave-length of about 544 millionths of a millimetre. The band at 572 of the scale is nearly as broad but not so dark; the one a little less refrangible than D is narrower than the others. The measures taken of the most refrangible band showed that it was at or very near the position of F in the solar spectrum. The light from a tube containing rarefied hydrogen, rendered luminous by the induction-spark, was then compared directly with that of Uranus. The band in the planet's spectrum appeared to be coincident with the bright line of hydrogen. There is no absorption-band at the position of the line of sodium. It will be seen by a reference to the diagram that there are no lines in the spectrum of Uranus at the positions of the principal groups produced by the absorption of the earth's atmosphere. On April 7th a faint comet was discovered by Dr. Winnecke. Dr. Huggins observed the comet on April 13 and May 2. It presented the appearance of a small faint coma, with an extension in the direction from the sun. When observed in the spectroscope, the light of the coma was seen to consist almost entirely of three bright bands. A fair measure was obtained of the centre of the middle band, which was the brightest; it gives for this band a wave-length of about 510 millionths of a millimetre. The position of the less refrangible band was only estimated roughly. The result gives 545 millionths. The third band was situated at about the same distance from the middle band on the more refrangible side. It would appear that this comet is similar in constitution to the comets which Dr. Huggins examined in 1868.

Mr. Rutherford has recently communicated to the Royal Astronomical Society a full description of the apparatus and arrangements he has devised for producing photographs of the sun and fixed stars; and at the last meeting of the Photographic Society Dr. Mann made a communication on the same subject, from which we condense the following particulars:—Mr. Rutherford's success has been mainly due to the photographic excellence of his telescopes —the object-glasses of his instruments having been corrected for photographic work, with peculiar care, under a plan of his own. The instruments, in their photographic equipment, are simply valueless for all ordinary purposes of astronomical vision, but they are as nearly perfect as may be for astronomical photography. The object-glasses of all astronomical telescopes are carefully corrected both for chromatic dispersion and for spherical aberration. But these corrections require a very material re-discussion and modification when the object-glasses are fitted to perform the highest class of photographic work. This further modification, in the case of his instruments, Mr. Rutherford has mainly accomplished by a very elaborate and refined process of step-by-step observation of imperfections in the photographic image, and step-by-step correction of the imperfections as they are noted in the successive observations. The photograph of the sun's disc presented to the Royal Astronomical Society is of exquisitely fine definition. The faculæ are clearly rendered; and a large group of dark spots are shown in their minutest detail, with the incipient bridging across one of them in the process of approaching disruption. The photograph of the Pleiades, however, is a much more remarkable object. This photograph was made with a telescope of 13 inches aperture, with six minutes' exposure, the telescope being, of course, carried during the time of exposure by a very carefully adjusted clock, so as to keep the image of each individual star upon exactly the same point of the sensitive plate during the whole period of exposure; and this service has been so marvellously well per-

formed by the clock that the image of each star is a clear round dot, without the slightest trace of elongation in the direction of the star's movement. There are about thirty stars registered in the photograph, clustered round Alcyone, the chief of the group; and it is very remarkable that the several stars are actually of different magnitudes, from Alcyone, which looks very much like a period of punctuation in printer's diamond type, to vanishing dots that can only be discerned by the help of a lens. The magnitude of the stellar disc is most probably a result of diffraction; but however this may be, there certainly it is. The stars on the photographic plate are not mathematical points, but little dots of different sizes, just according to the notion that the eye forms of them in looking on the group with an aperture of large diameter. There is one very ingenious piece of precautionary contrivance connected with the photographic impression of these stars well worthy of note. Mr. Rutherford's main object, in the labour he has been incurring in perfecting his process of celestial photography, is to afford a ready means of providing registers of the exact positions of the fixed stars which shall be altogether free from the possible errors of personal observation. Now it unfortunately happens that photography has an awkward and mischievous trick of making stars on its own account, which may readily be confounded with the photographic impressions of the proper host of heaven. Any speck of accidental imperfection in a photograph may be readily taken for the image of a star. It therefore becomes a matter of paramount necessity, when groups of fixed stars are to be dealt with for astronomical purposes, that some method shall be devised whereby the portraits of the stars shall be distinguished from the photographic accidents and star-ghosts. This object Mr. Rutherford has most efficiently and satisfactorily accomplished by the simple expedient of covering the object-glass of the telescope, and disconnecting the instrument from the clock-movement for a few seconds, after the six minutes' exposure necessary to give the image of a fixed star, and then again attaching the telescope to the clock and giving a second exposure of six minutes. The immediate effect of this is that every true star image is closely followed by its double—thus . .; in other words, every star photographically pourtrayed is a double star; and this process of re-duplication is obviously one which photographic imperfection, or accident, is quite unable to simulate. When once the photographic images of a group of fixed stars are impressed upon a plate, the measurement of their relative distances becomes a mere matter of mechanical operation. But in Mr. Rutherford's proceeding the relative positions of the stars is also fixed by securing upon the photographic plate a tracing of the direction in which some star moves across the plate, when the telescope is not carried by the clock-movements—that is, a tracing of a line coinciding with a circle of declination. Bright stars, such as Sirius and Vega, leave a distinct trail as they travel across the plate. In the case of fainter stars the same result is secured by giving a second exposure of the star after a brief interval. A line then drawn from the first image of the star to its second image gives the exact direction of the declination parallel. In the photograph of the Pleiades it will be seen that Alcyone has thus been made to leave a position-impression nearly half way across the plate from the re-duplicated image first registered, and that these successive images have been subsequently connected, for purposes of reference and measurements, by a visible pencil-line. Three large telescopes have already had their object-glasses corrected for photographic work under this plan of Mr. Rutherford's. The first on which the correction was made was composed simply of two discs of glass constructed under the ordinary arrangements of the achromatic combination, and had an effective aperture of 11¼ inches. A second, with an effective aperture of 13 inches, is now in use in Mr. Rutherford's observatory; and this large instrument has ingeniously been fitted to be used for ordinary astronomical vision, or for photographic work, by applying the photographic correction through the instrumentality of a separate constituent. The object-glass has such a figure that it is visually perfect, until it is thrown unto photographic equipment by the addition to it of a third lens of glass. It is primarily composed of the ordinary double lens, one of flint- and one of crown-glass. The photographic correction is all con-

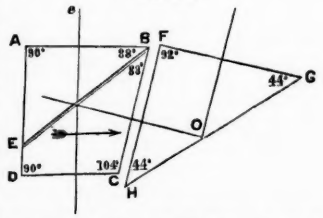
centred in and applied by a third meniscus lens of flint-glass, which is added to two other lenses when the telescope is to be used for photographic work, and which is taken away when the instrument is to be employed in the more usual work of visual observation. This arrangement is found to be so convenient, and to act so admirably, that there is no doubt it is the form which will henceforth be adopted in all important observatories, to enable their large equatorial instruments to be turned to account in occasional photographic work. The third telescope which has been photographically corrected is of the same form of construction, and has an effective aperture of 6½ inches. This instrument was made for the United States Government Eclipse Expedition, and was used by the expedition in Catania in December last.

Microscopy.—Messrs. Powell and Lealand have recently constructed a ⅛th objective, in which, in addition to the usual arrangement for correction of the aberration caused by the glass cover by separating the front lens, another collar is added which acts in a similar manner upon the posterior combination; the result is that the adjustment of the objective can be far more correctly made than by the old plan, and a greater thickness of cover glass can be used. The above object-glass worked well through a cover of 0·01 inch in thickness.

The erecting binocular microscope of Mr. J. W. Stephenson, although originally contrived for the purpose of dissection under low powers, is capable of performing well with objectives up to ⅛th, the definition with this power being but slightly impaired. For this purpose it is found convenient to omit the upper prism, which was added chiefly for the purpose of permitting the stage to remain in a horizontal position, while the bodies were conveniently inclined for observation. A modification of this upper prism also permits the employment of two pairs of bodies, which enables observations to be conducted by two persons simultaneously: the use of this latter form only involves the loss of a small portion of light, which is easily compensated for by increased intensity of illumination. This is decidedly the best binocular introduced since Mr. Wenham's.

Two binocular microscopes acting upon the principle of dividing the pencil by double refraction by means of a prism of calc-spar have been recently contrived. In one, the invention of Mr. F. A. P. Barnard, President of Columbia College, New York, a prism of calc-spar (Fig. 11), A B C D, cut to the angles indicated in the diagram, is divided at B E; the surfaces are separated by a small interval: in practice they are kept apart by a single thickness of tinfoil introduced at each of the angles. The prism, F G H, is of flint glass, with a refracting index of 1·56 or higher; it is placed with the side F H parallel to B C. A ray incident perpendicularly upon D C and doubly refracted by the prism is resolved into two rays, E and O, of which the first is transmitted and the second reflected by B E, passes perpendicularly through the two surfaces B C and F H, and is a second time reflected by G H, and finally emerges at right angles from the face, F G. The author states, that with a Wales's objective marked 1-30th but more exactly rated 1-25th, with the B eye-pieces, the "Providence" *Grammatophora* is resolved with great facility.

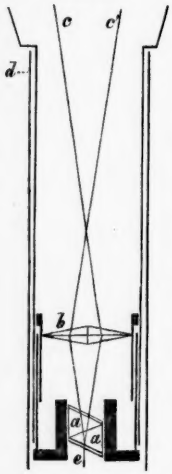
Fig. 11.



The binocular adaptation of Mr. C. D. Ahrens consists of a double image-prism of calc-spar of peculiar construction, *a a*, with a piece of thin parallel glass cemented on each end, which not only preserves the surfaces from injury, but also improves the definition; the ray *e* from the object-glass is divided in each portion of the prism into two rays, but only the extraor-

dinary ones are used, the ordinary being shut off; as there is a good deal of colour in these rays, it is neutralised by the double wedge flint-glass prism, of angle of 25°, *b*, placed over the double-image prisms, which also causes the rays to cross, the separated rays, *c' c*, then proceed to two symmetrically divergent tubes containing the eye-pieces. The tube containing the prisms and eye-pieces fits into the single body in the place of an ordinary draw-tube. The inventor states that the instrument answers well as a micro-polariscope, as the double-image prism takes the place of the analyser, and with much better effect than the usual Nicol prism, as it is worked with greater truth, but only one body can be used at a time unless there is a Nicol prism in one of the tubes, otherwise there will not be the same colour in each field. Mr. Ahrens also suggests that this binocular eye-piece might be used with advantage in the telescope, for by its means we could tell whether the object examined shone by its own or by reflected light.

FIG. 12.



Mr. B. T. Lowne, M.R.C.S., in describing the structure of the so-called "suckers" on the foot of the male *Dytiscus*, demonstrates that the production of a vacuum, as commonly supposed, has nothing whatever to do with their adhesion. Upon causing the insect to attach itself to the interior of an air-pump receiver, the "suckers" did not relax their hold upon the exhaustion of the vessel, but the adhesion was rendered closer by the exudation of the glutinous fluid, which is the real cause of attachment. In cases where the glue has hardened, the suckers are frequently torn away by the insect in its efforts to regain its liberty.

Prof. A. M. Edwards has examined a specimen of marble obtained near Thurman, about twenty-five miles from Saratoga, New York. The whole mass was found to consist of *Eozöon Canadense*. The material is said to occur in large quantities. The discovery is of great geologicial importance, as it indicates that the rocks belong to the Laurentian period, and the more so as the Canadian Geological Survey have only lately traced these rocks into New England —as far, at least, as Salem, Massachusets. Specimens of *Eozöon*, prepared by Dr. Carpenter, are in the cabinet of the Royal Microscopical Society.

Mr. Charles Cubitt, C.E., in the May number of the "Monthly Microscopical Journal," gives a statement of his views respecting the delineation of objects under microscopic observation. After criticising the drawings of several microscopical observers, which he refers to, and in which he detects various inconsistencies, he gives his own mode of proceeding, which is to make rough drawings of the object, then with the eye-piece micrometer to make all necessary measurements. From these data plans and elevations are prepared, and the perspective figure obtained by the process of *linear projection* well known to mechanical draftsmen. He selects as a subject for practical illustration the familiar *Melicerta ringens*, and in the plates accompanying his paper gives the plans and sections and the various lines needed in obtaining his finished representation, which is a marvel of patient application and decidedly accurate, but has, nevertheless, an inartistic stiffness. The process seems far too complicated and tedious for microscopists in general, and only attainable by those who have studied mechanical drawing in an engineer's or architect's office. The whole paper is written from a decidedly engineering point of view, but will do good in causing greater attention to be paid to accurate representation, although few besides its author have time or skill to carry out his views in their integrity.

Mr. H. J. Slack, F.G.S.,* gives an account of the "Optical Appearances of

* Royal Microscopical Society, April 5, 1871.

Cut Lines in Glass." Alluding to his paper on the "Cracks in Silica Films,"* he states that the chances of obtaining perfect illusions are increased by the amount of magnification and the perfection of the objectives employed. The false appearances look more real with well corrected objectives and careful illumination than with bad. Before commencing observations on ruled lines he recommends a careful examination of the edges of a number of thin cover squares, held together and viewed by transmitted light; the observation is to be conducted with a series of objectives commencing with an inch and proceeding upwards to a ¼th or ⅛th. Parts of the glasses' edges can easily be focussed to show the true form, but some portions a little in or out of focus will show beads, appearances like columns of Egyptian architecture, &c. Most of these optical appearances are sufficiently hazy or confused to give warning of their true nature, but some may be found so sharp and clear that they might easily mislead a practised observer. The lines used were ruled at varying distances: 1—2000", 1—3000", and 1—4000". The line cut on glass is defined by Mr. Slack as a furrow, more or less rough at the bottom and sides, and when viewed correctly under the microscope, has the appearance of a narrow depression less transparent than the adjacent spaces. It is difficult to obtain a correct view. The edges of a cut are often apt to appear as two raised lines. The ruled lines, examined in various ways, presented all kinds of appearances; for instance, the cuts appeared as rounded bands, half-round hollows, with rod-like ridges in the middle; flattish spaces, with narrow, raised edges. The true appearance was seen only in a single instance. Much is evidently to be done by careful microscopical examination, under varying circumstances of illumination, &c., of objects the true nature of which is known. The value of such researches is clearly shown in Mr. Slack's paper.

The Royal Microscopical Society has recently acquired an extensive collection of vegetable fibres, chiefly the production of India. The slides are mounted in duplicate, one dry, showing the fibre in its natural condition, the other in balsam, with the ultimate fibres separated, for polariscope observation, by which the structure is better displayed than by any other known means. The collection is similar to one prepared for the India Museum, and is likely to prove of value to those interested in textile materials. Many of the little known Indian fibres are strong, fine, and procurable in large quantities: there exists only the usual prejudice against new materials; many of them have for years been employed in India for fabrics and cordage, paper-making, and many other purposes.

The second part of Vol. 160 of the "Philosophical Transactions" contains the long expected account by Dr. G. W. Royston Pigott, M.A., &c., on "A Searcher for Aplanatic Images applied to Microscopes, and its effects in increasing power and improving definition." The author commences by giving a detailed account of various experiments on the definition of microscopic objectives, which he was led to undertake by an accidental appearance of black beads instead of the usual "!markings" on the *Podura* scale. At an early period of his researches he abandoned the usual test-objects on account of their real structure being unknown, and formed a test by diminishing some known object, such as a thermometer scale, by means of a high power objective placed in the position of the condenser beneath the stage of the microscope. This aërial or aqueous image, as dry or immersion objectives were used, formed a test readily showing the quality of the definition. Some of the results are surprising, and such as would hardly be expected from what the author calls a "*very fine eighth*." When the draw-tube was used to increase magnifying power by lengthening the body, it was found that the increase of aberration was in much greater proportion than the increase of magnification. Experiments conducted with the view of ascertaining the nature of these aberrations determined that false images, "*eidola*," existed above and below the best focus, and that under certain conditions these *eidola* might become, so to speak, mixed with the true image, giving rise to a great amount of confusion and

* Monthly Microscopical Journal, vol v., p. 14.

necessary liability to errors of interpretation. Another series of experiments on the effects of traversing combinations of lenses placed within the body of the microscope suggested the idea of "searching the axis mechanically for aplanatic foci;" the trials ultimately resulted in the "Aplanatic searcher" thus described by Dr. Pigott:—"A pair of slightly overcorrected achromatic lenses, admitting of further correction by a separating adjustment, are mounted midway between a low eye-piece and the objective, so as to admit of a traverse of 2 or 3 inches by means of a graduated milled head. These lenses are conveniently traversed within the draw-tube, and can be brought to bear within 4 inches of the objective, or at a distance of 10 inches. The focal length of the combination forming the aplanatic image-searcher may vary from 1½ inches to ¾ of an inch. The latter applies more effectively to low objectives, when it is desirable to obtain extraordinary depth of focal penetration and vision through very thick glass,* as with a ¼ inch giving 700 diameters with a C eye-piece. It should now be stated that the searcher may be employed with very different intentions. Thus—when it is desirable to view an object through a very thick refracting medium, the searcher is brought as close as possible to the objective, which action lengthens the focus of the objective; and the same thing is necessary when the observer wishes to throw the *eidola* of an upper structure above and away from the true image of the lower but contiguous stratum, as when the lower beads of the *Podura* are required, or when it is required to give additional negative aberration to an objective too positively corrected, in which the front glasses are already forced into a dangerous proximity. On the contrary, when the searcher is traversed the opposite way, the *objective lenses* require to be brought nearer together; the instrument is then more adapted for viewing objects or particles lying in the upper plane of a complex structure, throwing the *eidola* of the lower layer below that layer itself, and so leaving the upper stratum less disguised by the false images of the lower. In intermediate cases, where greater penetration or focal perspective is required, with a thin glass cover, the objective lenses must be proportionately separated by an increased interval, the searcher being traversed towards the objective; and in general confused images of both upper and lower strata can be obtained by opposite arrangements. The most brilliant definition is generally obtained when the searcher (a little more overcorrected) is used as close to the objective as possible. The overcorrection of the searcher is increased by separating its component lenses according to the divisions upon the sliding tubes of the searcher. The use of this instrument will be facilitated by first setting the microscope for ordinary use without the searcher, adjusting an eye-piece, the focus, and screw collar to the most distinct vision, and then applying the draw-tube containing the searcher placed at a point nearest to the eye-piece. As the searcher is traversed towards the objective, the lenses of the objective may *require separation*. The change in the general aberration is shown by the divided index of the milled head actuating the movement of the searcher. The power obtained is in general from two and a-half to four times greater than that given with the third eye-piece, C, of 1 inch focal length; with a very fine eighth of Messrs. Powell and Lealand's *new* construction a clear and satisfactory definition of the beading of the *Pleurosigma formosum* was exhibited to them, by means of the aplanatic searcher, at a power estimated at 4000 diameters. Several inferior objectives have acquired a fine definition by the application of the searcher. This paper, perhaps, will, hardly be complete if I omit to add that the instrument will be most effectively employed by considering it as a conjugate portion or integral part of the objective itself, in which the minute traversing adjustment of the objective lenses finds its counterpart in the more extended, and, therefore, more delicate adjusting traverse of the searcher itself. So that, in short, during minute microscopical research, each adjustment should be intelligently applied, according to the nature of the research in hand. The indications of the one adjustment should be employed to verify those of the other. Correlative movements, by the aid of the searcher, may introduce aplanatic images, whilst a violation of

* Nearly one-fourth of an inch thick.

their correlation will exhibit deformity." These instructions relating to the use of the searcher have been quoted at full length, as some of the failures in the use of the searcher have probably been caused by ignorance as to the mode of its application, and as a natural consequence, observers disagree as to its efficiency. The editor of the "Monthly Microscopical Journal" justly remarks:—"There will, doubtless, be many cheap imitations, which, it may be feared, will only caricature the properly constructed instrument, which must be regarded as a new aid to microscopical research." It is unfortunate that so elaborate a paper should give no constructive details to aid opticians in manufacturing the instrument; the form of the lenses can only be guessed at from the figures, no mention being made of their curves, or the quality of glass employed. This is a great omission, which it is to be hoped that Dr. Pigott will remedy by a future publication. Experiments with a properly constructed searcher, in the hands of microscopists accustomed to high power observations, are needed before the true value of Dr. Pigott's invention can be ascertained.

HEAT.

The re-solidification of broken ice at temperatures above the freezing-point has its parallel phenomena under like circumstances in the cases of many other substances. Some of these are well-known, and at times exceedingly troublesome to workmen in certain manufacturing businesses where rapid liquefaction is a desideratum, but they have not been noticed in any scientific journal. Mr. C. W. Vincent has communicated to the "Chemical News" some instances of these apparent parallels to regelation. Good black rosin, free from turpentine, when subjected to pressure in a mould, or otherwise, at ordinary temperatures, becomes completely pulverised, its particles showing no cohesive power whatever. When the temperature of the mould is raised considerably above the melting-point of rosin, on pressure being applied, a different result ensues; the mass becomes at once solid to the core, the outside alone showing signs of liquefaction. When rosin has to be melted for manufacturing purposes, if even for a few minutes only the workmen neglect to stir, the whole mass becomes completely solidified, and the liquefaction is only carried on at the exterior of the mass, which, when pounded by his stirrer, breaks up without giving the slightest preference to the previous lines of fracture. Pitch, at melting temperatures, gives the same result as rosin, but becomes viscid so much sooner, that though coagulation is readily affected, yet the perfect junction of the broken pieces into a solid mass is not so readily obtainable. The gums used in the manufacture of varnishes, such as dammar, copal, anime, shellac, &c., give similar results. Whilst being run down at high temperatures, they settle together and cohere (if their surfaces be clean) without the possibility of again separating them in the same places; and this while the exterior of the mass is rapidly melting into liquid of almost the fluidity of oil.

Aëronauts have frequently observed a phenomenon which they term as *le ballon fume sa pipe*—that is to say, the gas the balloon is filled with issues from the lower opening of the balloon in the form of a whitish smoke. Dr. de Fonvielle supposes that this phenomenon is due to the cold produced by the increase of the volume of the balloon while rising rapidly upwards. Indeed, for a decrease of pressure of 1 millimetre per second of time, the increase of volume is equivalent to that produced by an elevation of temperature of two degrees for a height where the mean pressure would be 660 millimetres of mercury. A similar phenomenon takes place in every instance of the rapid rising of a mass of humid air, which, while becoming dilated, does as the balloon and *fume sa pipe*—that is to say, leaves behind it, in the shape of more or less dense cloud, the watery vapour which it contained previously in the shape of a diaphanous invisible gas.

A project for obtaining a larger amount of steam from a given quantity of fuel has been patented by Mr. C. F. Varley and T. A. Rochussen. When coal is burnt the solid coal is turned into gas, a large portion of heat becomes

latent, and is wasted by volatilising the solid. When zinc, iron, or manganese are burned, the resulting oxide is a dense solid; little or no heat is wasted, as it is not turned into vapour. In addition to this, the inventor obtains the cosmical heat latent in the oxygen of the atmosphere, and the result is that one pound of zinc will evaporate more than four times as much water as one pound of coal, the advantage of which on long sea voyages is obvious. The zinc or other metal thus becomes a vehicle of power, much larger than can be obtained from the same weight or bulk of coal, and the oxide of the metal may subsequently be economically reduced at any convenient place where coal is accessible. The following is the manner in which it is proposed to carry out this invention:—The furnace of the boiler is divided into two or more parts, first, the hearth or grate on which the metal is burned (in this description we will confine ourselves to the metal zinc); secondly, a chamber behind the hearth to collect the oxide. In the case of tubular boilers, the heated gas from this chamber is made to circulate through the tubes. The furnace has the bottom and sides, and sometimes the top also, of brick, fire-clay, or any other refractory substance. The air is admitted over the combustible metal, or by a blast through the same; in the latter case pipes or tuyeres are built in the bottom or sides of the furnace.

The extinction of fire by gas is probably about to be carried out in New York, a company having been proposed for this object. The following is the purport of the scheme:—The company proposes to build, in some central location, a reservoir similar to the gasometer of a gas-house, of sufficient capacity for all emergencies, and run a four-inch pipe therefrom in every direction through the streets, having in front of every door a valve, to which a small rubber hose can be easily attached, by which the stream of gas can be directed to any room on fire. To furnish such a reservoir, with its necessary retorts for making gas, and laying 200 miles of pipe through the city, will come within a cost of 250,000 dollars, and the company claims that the saving of property which water destroys and gas leaves untouched, would more than pay the entire cost in one year, to say nothing of the saving of the buildings. From the reservoir, through the pipes, carbonic acid gas will be forced to the endangered building. Repeated experiments have shown that fire cannot burn in an atmosphere containing one-fifth part of its volume of this gas, and that its presence does not injure the finest fabrics, or discolour the most highly polished plate; consequently it does no damage to goods or furniture, which water destroys. It can be manufactured at so low a price that the flames of a moderate-sized building on fire could be extinguished for a few dollars. Its use has been frequently endorsed by insurance companies. Up to the present time the difficulty in making use of carbonic acid gas for extinguishing fires has been found in the production and application of this gas in sufficient quantities for obtaining a practical advantage at a real fire. Experimentally the presence of this gas in quantity proportionate to a flame has been found to insure its sudden and complete extinction; but until within the last few months no plan has been put in operation to make this power available. In 1851 a coal mine in England, that had been on fire for thirty years, and had extended over twenty-six acres, was completely extinguished by injecting only 8,000,000 cubic feet of carbonic acid gas. Water and chemical solutions had been tried in every way and quantity for years upon these burning acres of coal, without any perceptible effect, for the intense heat would drive them back as a volcano sends out its lava into the heavens. The loss by water, caused by the engines playing after a fire is out, or nearly so, is very great. Once in operation, a steam fire engine is frequently as destructive as the conflagration itself. If the new method succeeds and is generally adopted, many of the calamities and losses that are yearly chronicled will cease. As the pipes will be immediately laid and a reservoir built, it is expected that very soon the new apparatus and plan will be practically tested.

The effect of cold upon iron and steel has formed the subject of a numerous series of experiments which have recently been communicated to the Literary and Philosophical Society of Manchester. The result of these experiments

appear to be that these metals were not rendered brittle by low temperatures. Most of the experiments of Joule, Fairbairn, and Spence were made by steady pressure, and therefore cannot be considered reliable when percussive force is brought into play. That there is a difference in these effects, so great that no relation between them can be determined, any one may convince himself by contrasting the tensile strength of glass with its extreme frangibility under percussion. Mr. Brockbank, whose paper drew forth the opinions referred to, took the ground that iron and steel were more liable to break in cold weather, and based his opinion upon percussive experiments. It is obvious, therefore, that his opinion has no weight upon the subject of tensile strength as affected by cold, but it is of great value as confirming experiments previously made to ascertain the effect of cold upon iron and steel subjected to percussion, experiments of which Mr. Brockbank was apparently ignorant at the time his paper was prepared. The "Scientific American" has drawn attention to some previous experiments in this direction. In 1869, a "Treatise on Iron and Steel," by Knut Styffe, was published in London, from a translation by Christer P. Sandberg. The translator, however, took issue with the author upon this very question, and denied the applicability of Styffe's deductions, from tensile experiments, to percussion in cold temperatures, founding his denial upon experiments performed by himself in Stockholm under the authorisation of the State Railway Administration of Sweden, in 1867. The results of his experiments prove that at 10° F. rails will not sustain much more than one-fourth the blow that they will at 84° F. The method of performing the experiments, as well as the details of each, are given in tabulated form in a voluminous appendix to the translation of Mr. Styffe's treatise. Mr. Sandberg concluded from his experiments that the brittleness of iron and steel under low temperatures is due to phosphorus present in the metal, and that with purer metal the results would have been different. It is evident that this subject is imperfectly understood, even by the highest authorities, and further extended investigations, with all kinds of iron and steel, must be made before the general effect of cold, as inducing brittleness under percussion, can be affirmed. Meanwhile, it seems to be well settled that the tensile strength of iron and steel when tested by stretching is not lessened by low temperatures. On the contrary, it would seem from Mr. Spence's experiments to be increased rather than diminished.

Any light that can be obtained on spontaneous combustion adds not a little to the value of property. Many of the fires charged to incendiarism are really owing to spontaneous combustion, so called. The editors of the "Boston Journal of Chemistry" give some cases illustrating this subject which have come under their own experience. Within the past year, twenty-eight rolls of cotton cloth in a large dyeing establishment were dyed black, and were delayed a few days before they could be starched and finished. Two of these rolls were discovered to be on fire—not in flames, but in a smouldering condition, or charred into tinder; a third roll was so hot that hands could not handle the cloth, and the wooden roller upon which the cloth was wound was heated almost to the point of ignition. The rolls of cloth destroyed were the first dyed, and consequently had been longer exposed than the others, which in a measure explains why all the rolls were not in the same condition. In the dyeing, the first rolls were dyed without washing, by an oversight of the dyer. This is the point of importance, as the chemical salts were left in the cloth. Logwood, potash, sulphate of copper, and sulphate of iron constituted the dye, and we suggest this explanation as the probable cause of the fire. The potash and sulphate of iron change to sulphate of potash and protoxide of iron, and by the absorption of oxygen from the atmosphere or from moisture in the cloth, this developes sufficient heat to reach the point of ignition.—A fire was discovered in a silk-mercer's shop in London. The fire originated in a lot of black-dyed silk, and was discovered, as in the first instance, before flame had burst out. The conclusion reached was that it was not safe to have black-dyed silk in large masses, and that each piece ought to be so placed as to allow free circulation of air. It is very probable that the explanation of the combustion is the same as in the preceding case.—In trying to get rid of rats in

a dwelling-house, the floors were taken up, in order to cut off their ingress, if possible. The box that held the hot-water pipes was found to be a favourite resort for the vermin, and had actually been on fire. The sides were charred, but there had not been sufficient air to sustain combustion. Upon investigation as to the cause of the incipient fire, we are not long left in doubt, for a store of remnants of greasy cloths used in washing dishes was found, which had been brought by the rats from the kitchen. Some of these were charred, and the others were well saturated with grease and oil. This fire was a distance from the kitchen range of at least forty feet. It would have been natural in all these cases, if the real causes had not been apparent, to attribute the origin of the fire to incendiarism. The introduction of coal oils for the lubrication of machinery has very materially reduced the number of fires from spontaneous combustion, owing to the fact that the coal oils do not absorb oxygen.

The employment of the oxyhydrogen blowpipe for attacking safes has recently been the subject of some careful experiments; the result being that, whilst it is a powerful auxiliary to the drill, it cannot be used alone with success. The temper may be drawn, in time, from a steel plate an inch thick, by the use of the blowpipe, so that the plate may be drilled. It may also be burned quite through when operated upon singly; but it is difficult to do this with iron plates, which burn less easily, and also conduct heat away from the point against which the flame is directed as rapidly as the steel. Spiegeleisen burns with even less facility than ordinary iron. The flame directed against the corner of a fragment of spiegeleisen fused it, but, after continued action, only produced a comparatively small amount of the oxide of iron, which coated the bead formed. The fused metal, on cooling, was as hard as before. This material, in fact, depends for its hardness upon its natural composition, and not upon any process of tempering, so that mere melting does not change its character. It would, however, require apparatus not available to burglars to melt a hole in the centre of a spiegeleisen plate. It follows, therefore, that while iron plates and steel plates may be successively penetrated by the use of the blowpipe, as practically capable of use in the hands of burglars, the spiegeleisen plate, which practically resists drilling, defeats the use of the instrument as an adjunct to the drill. Mr. Dickinson, of New York, manufacturer of carbon points for drills, &c., states that these points will not drill spiegeleisen, except by the use of appliances for obtaining speed, which cannot be used by burglars, and that to drill it at all would be a work of so much time as to prevent its adoption for safe-breaking. The rate at which, by the alternate use of the blowpipe and drill, a hard steel plate can be penetrated, is, according to Mr. Farrell, about one inch per hour; the drawing of the temper in advance of the drill occupying about two-fifths as much time as the drilling. It is found that the alternate use of these instruments enables more rapid progress to be made than when it is attempted to draw the temper entirely through the plate at a single operation.

A discussion on the subject of the congelation of bisulphide of carbon has been going on, for some time past, in the German chemical journals. Dr. Wartha considers that its congelation, which, according to the treatises on chemistry, requires a temperature of $-90°$ for its solidification, may be easily effected by directing a very rapid current of dry air upon the surface of the pure liquid contained in a glass vessel. If a thermometer be plunged into the bisulphide of carbon during this operation, a snowy crust will be noticed covering the sides of the vessel and the thermometer, even before the temperature has become 0°. The temperature then rapidly descends to $-18°$, and a white mammellated mass rises to the surface, and sometimes even stops up the tube for conducting the air. Soon all the liquid disappears and the thermometer commences to rise again up to $-12°$, where it remains stationary as long as the bisulphide of carbon is solid. In this state it presents the same phenomena as solid carbonic acid. Dr. Ballo has verified all the facts stated by Dr. Wartha, but finds that the solid substance obtained is not really solidified sulphide of carbon (frozen sulphide), but is a hydrate of the body alluded to, containing about 19·14 per cent of water. In a subsequent paper he states

that he has obtained what he calls chloroform snow and iodethyl snow, by directing a rapid and moist current of air upon these fluids contained in shallow basins. As regards the real nature of these solid substances, his experiments are not yet quite finished.

The freezing-point of mixtures of glycerine and water has been examined by Mr. C. Bullock. The glycerine used was common, sp. gr. = 1·250; the quantity of water, in all experiments, 1 gallon. Temperatures Fahr., with ½ pint, fuses at 30°, 1 pint at 24°, 1½ pints at 18°, 2 pints at 10°, 3 pints remain fluid at 3°. These determinations are of considerable value, as mixtures of glycerine and water are frequently used in cases where a mobile liquid is required at temperatures where water would be solid,—*e. g.*, in wet gas meters, ice machines, &c.

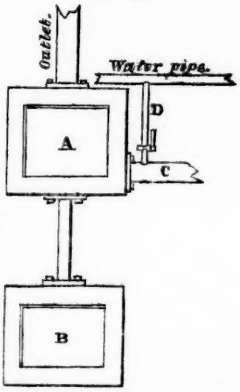
An ammonia engine has been designed by Mr. Emile Lamm, in which ammoniacal gas is used as a substitute for steam or air. Ammonia, at the temperature of our atmosphere, is a permanent gas of well-known pungent odour. Its density is 596; air being 1000. It may be liquefied by great pressure, and in the fluid state its density, compared with water, is ·76, or about one-quarter lighter than that liquid. Its vapour, at 60° Fahrenheit, gives a pressure of 100 pounds to the square inch, while water, to give an equivalent pressure, must be heated to 325° Fahrenheit. The volume of ammoniacal gas under the above-named pressure is 983 times greater than the space occupied by its liquid, while steam, under identical pressure, occupies a space only 303 times greater than water. If, therefore, by the application of equal quantities of heat to the specific heat of equal weights of steam and ammonia, such great difference in their relative volume is produced, it is evident that the cost of ammonia, as a motive power, would be one-third that of steam. This immense volume of the gas, compared to other gas or vapours, shows a decided exception to the law of Mariotte, relating to the expansion of gases, greatly in favour of ammonia as a motive power. The latent heat of ammoniacal gas is 880, that of steam being 990. Ammonia is a powerful alkali. Its action upon most of the metals is *nil*; still, strange enough, it acts slowly upon one of the metals of the second class—*i. e.*, copper. Upon metallic iron, which is a metal of the third class, its action is absolutely *nil*; but it readily dissolves carbonated oxide. Ammoniacal gas is absorbed by water with avidity, one volume of water at 70° Fahrenheit absorbing 500 volumes of the gas. The water becomes specifically lighter, whilst its volume is augmented about one-third. As the absorption of the gas goes on, the water becomes heated, and the latent heat of the gas reappears as sensible heat. It is in this property that water possesses of absorbing so large an amount of the gas, and of becoming heated while absorbing it, that the practicability of using ammoniacal gas as a motive power rests, for it must be well borne in mind that the only agency for producing motive power is heat. It matters very little that liquefied ammoniacal gas boils at 40° below zero—72° below the freezing-point; it requires as much heat comparatively as water itself, for its complete and rapid evaporation, when acting as a motive power in an ordinary engine. But ammoniacal gas possesses the remarkable property, from its affinity for water, of being able at any time after its condensation into a liquid to reproduce, at a distance from the receiver where it was condensed, a force equal to the heat which was necessary for its condensation. This reproduction is owing to the fact that the latent heat of the gas appears anew in water of re-absorption, and is re-transferred to the liquefied gas. The re-transfer of the heat to the liquefied gas takes place, through metallic tubes of which the reservoir is composed, from the water of re-absorption which surrounds them, and is similar in its operation to the action of fire in the furnace of a steam boiler. An engine driven by ammonia instead of steam has actually been in use for some time in America, for the purpose of driving street cars. In the last trip of seven miles made by a street car driven by ammonia, the engine used on the car was equal to two-horse power; the ammonia expended during the trip amounted to 1·16 cubic feet. The latent heat

of ammoniacal gas being 880, the whole heat expended during the trip would have been sufficient to raise 84 gallons of water from the temperature of 83° Fahrenheit to the boiling-point, 212°. If for supplying this heat it was necessary to rely upon the heat absorbed from the atmosphere, even on the hottest day of summer, it is certain that a car loaded with passengers could not run more than three hundred yards without being obliged to stop, and wait at least fifteen or twenty minutes, in order to acquire sufficient heat from the atmosphere to again run an equal distance. But the mechanical equivalent of heat expended in working the engine by ammoniacal gas, one-seventh of the total heat, according to the best physicist, is fully made up by the extra heat of chemical combination becoming also sensible at the instant of the re-absorption of the gas in the water surrounding the tubes. The register of the pressure-gauge at the moment of starting, and during the trip, shows conclusively that the extra heat compensates all losses. The mean pressure during a trip remains essentially the same, if we take into account the time necessary for the transmission or equalisation of heat between the reservoir containing the liquid gas and the re-absorbing water in which it is immersed. On the last trip made with a street car, the gauge registered at the start 150 pounds; lost in pressure, in making 1¼ miles, 30 pounds; gauge then registering 120 pounds. Upon stopping for ten minutes it registered 180 pounds, thus gaining 60 pounds. On stopping at the end of the trip of seven miles, the gauge remained stationary at 176 pounds. The above variations can easily be explained. It is impossible to construct an apparatus in which the transmission of heat would be instantaneous; therefore, in rapid travelling, the reservoir of liquid ammonia becomes cooler than the water of re-absorption outside; but the difference never exceeds a certain point either way, the extreme of which, in the last trip, starting with 150 pounds, was a difference of 45 pounds. The maximum of pressure during one of the warmest days of summer was then 195 pounds. When the reflection is made that this tension was exerted in a boiler, which stood 600 pounds hydraulic pressure, no other commentary is necessary as to its entire freedom from danger; and this is easily explained, for as the temperature between the reservoir of liquefied ammonia and the water of re-absorption diverges, the transmission of heat becomes more or less rapid, in the direct ratio of their degree of divergence,—on this principle, that the production of heat in the re-absorbing water is equal to its reduction by evaporation in the reservoir of liquefied gas. One other advantage which ammonia possesses over steam is the fact that its vapour, not being condensed at the usual temperature of our atmosphere, does not, like steam, at a low temperature, suffer condensation, either in the cylinder of the engine or when used at a distance from the boiler. The cheapness of ammonia as a motive power, when compared with steam, is owing to the fact that one steam-engine, if it could be made to propel one hundred street cars with ease, would be much cheaper than one hundred steam-engines, each requiring a separate fire and an engineer, besides the regular conductor of the car; but the case is far different with ammonia, as a single engineer at the station can superintend the supplying of two hundred cars with liquefied ammonia, in sufficient quantity to run any distance within the limits of a large city, by means of a single fire under the stationary boiler in which the ammoniacal gas is liquefied. Further, liquefied ammonia can be compared to a bottled-up power, which can remain in a reservoir for months, or even years, and be transported anywhere in any desirable quantity; and then at once, without any further preparation, can be used for any purpose desired, and by the simple turning of a tap can be made to act as powerfully as when first liquefied. The other advantages of ammonia as a motive power are: First, its perfect safety—for the reason that its power is exerted at a very low temperature, and consequently its influence in weakening the tenacity of the iron vessel containing it is trifling, when compared with the destructive effects of a high temperature: for it is a fact, known beyond dispute, that the main agency which causes those frequent and deplorable explosions of steam-boilers is the high heat they require for the production of an effective force. Second, for marine purposes there is no doubt that it will conquer for itself a high place before long, for this reason—

that, with an initial pressure of 20 pounds or 25 pounds in a boiler, it would give an effective pressure of 150 pounds on the pistons of the engine.

Mr. J. Sutherland has described an ingenious modification of the common suction pump, to be employed for the pumping of hot liquids where there is considerable difficulty in getting a sufficient flow of the liquid when its temperature is at all near the boiling-point. This result, of course, arises from the well-known fact that liquids boil at lower temperatures exactly as the pressure on its surface is decreased. The pump itself was simply a steam cylinder, having a set of ports opened and closed by slide valves, and producing an exhaust by means both of the up and the down stroke. This arrangement gave excellent results so long as the liquid was comparatively cold, throwing a continuous stream 2½-inch bore or about 10 to 12 tons spent-lye per hour. The cause of failure was due to the exhaust produced by the pump being as rapidly filled up as formed by the vapours generated from the liquid, brought to a state of ebullition by the decreased pressure produced in the pump connection. It then occurred to Mr. Sutherland, that if he could introduce a fine spray of cold water at the inlet of the pump it would condense the troublesome vapour and allow the pump to do its work. A water-pipe passed along the wall within a foot or two of the inlet, and, between the two, a connection was opened with a ¼-inch lead pipe having a stop-cock to regulate or shut off the water supply. The end fitting into the cast-metal pipe was fitted with a small rose, in order to spread the water on entering. The accompanying sketch shows the arrangement. A is the pump cylinder, B the steam cylinder, C a 3-inch inlet, D ¼-inch pipe introduced on the upper surface of the inlet pipe. It is imperative that it be introduced on the upper surface of a horizontal pipe, as it is there the first vapour will gather and be exposed to the condensing action of the spray. Were the cold water introduced at the lower surface of the pipe it would mix with the hot liquid, and, in all probability, its action would be almost *nil*. The application proved a thorough cure, and not more than about ⅛th of an inch bore of water was necessary to set the 3-inch inlet in full action.

FIG. 13.



ELECTRICITY.

At the meeting of the Royal Society of Edinburgh, held on the 29th May, Dr. R. M. Ferguson described and exhibited a magneto-electric machine of the following nature:—The revolving Siemens's armature consists of one piece of soft iron, with two grooves cut in it, the one at right angles to the other, for the reception of two coils of insulated wire, one of which, on the motion of the armature, gives off a current to excite the electro-magnet, between whose poles it rotates, and the other gives off an external current. It is in effect a modification of Ladd's ingenious application of Siemens's and Wheatstone's principle of magnetic and electric reciprocity, only instead of two armatures placed in length and moving on the same axis, with their coils at right angles, a single double coiled armature in this instance is employed. The grooves are of unequal sizes, the larger being four times more capacious than the smaller, and the larger coil, which furnishes the external current, has its wire twice as long and twice as thick as the wire of the smaller coil and groove. The commutating collars at each end of the armature, against which the springs press, can be moved round and fixed at any angle with the planes of the coils. The core of the electro-magnet, made of boiler plate, is 11 inches long, 9 inches high, and 6 inches between the sides, and the revolving armature within is 11

inches long, and 2½ inches in diameter. The peculiarity of the action is, that the hand in driving feels little or no resistance if the circuit of the larger coil be of small resistance; but it has to exert great force to maintain the same velocity in a circuit of great resistance. The driving power must therefore be in some measure proportional to the work done in the outer circuit, and when this circuit is broken, and the electric action is limited to the reciprocal action of the small coil and the electro-magnet, it is excessively hard to turn the handle of the machine. With the same rotating speed a current approximately of the same strength can be maintained in any external circuit, provided only the driving power be sufficient. At a certain rate of motion, for instance, the hand feels it has little to do in melting or igniting two inches of a soft iron wire $\frac{1}{25}$ of an inch in diameter; to effect the same with six inches of the same wire, one hand finds enough to do, and when eight inches are in circuit, both hand are needed to bring it to ignition. Even with the eight inches, little resistance is felt till the heating of the wire ensues. When the external circuit is made through the coils of an electro-magnet a few pounds in weight, it is impossible to keep up a high rate of speed, as the point of saturation of the magnet is never reached. But with a voltameter it is different; the limit of resistance is soon reached, and the driving resistance does not rise beyond a moderate amount, so that the quantity of explosive gas varies with the work of one hand from 3 to 6 cubic inches only, according to the velocity of motion. The principles involved in the action of the machine, which were discussed at some length, seem peculiar and interesting.

Professor A. A. Mayer, of Pennsylvania, has invented a new method of fixing, photographing, and exhibiting magnetic spectra. He first coats a clean plate of glass with a solution of shellac in alcohol, in the same manner as a photographic plate is coated with collodion. After the plate has remained a day or two in a dry atmosphere it is placed over the magnet, so that the under surface of the plate just touches the magnet. Fine iron-filings of Norway iron, which has been repeatedly annealed, are now sifted uniformly over the film of lac. The spectrum is then produced on the vibrating plate by letting a light piece of copper-wire fall vertically upon it at different points. The plate is now cautiously placed on the end of a cylinder of pasteboard, which serves as a support in bringing it quite close to the under surface of a cast-iron plate, which has been heated over a large Bunsen flame. Thus the shellac is uniformly heated, and the iron-filings sink into the softened film. When the shellac is hardened it fixes the spectra. When photographic prints are to be made from the plate, the heat is allowed to act until the metallic lustre of the filings has disappeared, and the film appears quite transparent; but when the plate is to be used as a magic-lantern slide the heating is not carried so far. Many plates have been made by this process, showing the action of single magnets of various forms and of juxtaposed bars, as well as the effects of electric currents led by wires through holes drilled in plates. Among the most interesting are those spectra exhibiting the inductive action of magnets on bars of soft iron and the interaction of magnets and electric currents.

A lecture experiment to prove that mercury is heated while a galvanic current passes through it has been devised by Dr. F. C. G. Müller. Take a glass tube 6 c.m. long and about 6 m.m. diameter; heat it before the blow-pipe, and, while soft, reduce its diameter in the centre to ½ m.m., and next bend it to a U-shape; fill it with mercury, fasten it in a clamp, and afterwards dip the wires of a galvanic battery in the metal. This having been done, the mercury in the narrowed portion of the tube will be observed to boil rapidly, while a continuous series of sparks will be also exhibited.

The uses to which a clock indicating correctly the same time in as many different places as may be desired can be applied are manifold. In astronomical observatories, public offices, manufactories, institutions, and for railways such a clock would be of extreme utility. Sir Charles Wheatstone's latest patent is for a magneto-electric clock driving sixty or seventy other clocks, and dispensing with all voltaic batteries or other common causes of failure. The system consists of a motor- or driving-clock and as many clocks in circuit

as may be desired. The driving-clock has, like an ordinary clock, a weight, multiplying wheels, escapement, and a pendulum. For the ordinary pendulum-bob is substituted hollow coils of insulated wire, oscillating over the poles of two compound permanent magnets. An electric current is produced at each oscillation of the pendulum, which current is in one direction as the coil passes from left to right, and in the opposite direction when it proceeds from right to left. These alternately inverted currents are transmitted by conducting wires to the clocks in circuit, in which the escapements usually employed in telegraphic clocks are dispensed with, and the motion of the hands rendered perfectly continuous. The currents proceeding from the driving-clock are caused to pass through a horizontal coil of wire similar to a galvanometer-coil, in the centre of, and above and below which, is an astatic series of magnetised needles fixed on the same arbor, carrying a pinion in connection with the train of wheels communicating its motion to the hands. The needles are propelled for half a revolution by the current which is produced while the pendulum of the driving-clock is moving from right to left, and another half revolution while the pendulum passes from left to right, thus describing a whole revolution in one second, if half a second is required for each beat of the pendulum. The time when the needles after each half revolution arrive at a position at right angles with the wire of the coil, corresponds exactly with the moment when the pendulum of the driving-clock has arrived at its greatest deflection from the perpendicular. In all former telegraph clocks the current has had to overcome the *vis inertia* of the magnetised needles of the secondary clocks, thus entailing a great loss of power. But in this clock the currents are aided by the momenta of the needles, so that extremely weak currents arriving at the proper period are sufficient to impart motion to the train of wheels. Thus the needles are aided in their rotation much in the same manner that the oscillations of a heavy pendulum can be maintained by well-timed puffs of breath. The driving-clock can be regulated by a standard clock at any distance, it being understood that the regulator possesses one of the usual contrivances for the momentary completion of a voltaic circuit, or, if in proximity to the driving-clock, that a mechanical connection may be established. The regulating is effected by a series of very simple mechanisms, by which the pendulum of the driving-clock is made to shorten or lengthen itself automatically, as the clock has lost or gained. The advantages of employing magneto- instead of voltaic-electricity as the maintaining power of the secondary clocks are obvious. In every case where the source of power is a voltaic-battery, there must necessarily be established a certain number of contacts, and at each of these interruptions of the circuit a spark, no matter how minute, passes between the metallic points. This spark is nothing more nor less than a piece of the metal in a state of incandescence, and the result of this rapid oxidation of the points is the stoppage of the clock in a few weeks. By employing magneto-electricity, Sir C. Wheatstone has obtained the inversion of the current in a closed circuit, and consequently avoided the chief cause of failure hitherto. Moreover, the driving-clock can be made to keep time to any degree of nicety, independently of a regulator.

Another invention of Sir Charles Wheatstone is a magneto-electric counter for registering the number of revolutions or oscillations of any machinery, or the number of visitors to a public building, &c. The instrument consists of a permanent magnet, with soft iron prolongations, on which are wound coils of insulated wire; the currents being induced by the removal and attraction of an armature in connection with the machinery or door whose movements are to be registered. These currents are conveyed to the register, where they act upon suitable electro-magnets, causing the deflection of a magnetised needle. This deflection is continued by a train of wheels to an index-hand, which points successively to the figures on a dial in the usual manner. The register can thus be placed at any distance from the motor-magnets, out of reach of any ill-disposed person. The door, if the counter be so attached, remaining open, the index of the register points to a space intermediate to the two figures, so that the person in charge of the machine can at

once see what is wrong. The instrument has been successfully applied to register the number of revolutions of the screw-shafts of some of our largest steam-vessels, and also to indicate in the editor's room the number of impressions of a newspaper press.

It is certainly desirable that such an important art-science as Telegraphy should have its representative institution, and the advent of the Society of Telegraph Engineers will be gladly welcomed by the profession. The Society held its first elective meeting on the 31st ult., when Charles W. Siemens, Esq., C.E., F.R.S., was elected President, and Lord Lindsay, and F. J. Scudamore, Esq., Vice-Presidents. Among the Council are Sir Samuel Canning, Professor Foster, B.A., F.R.S., Major Stotherd, R.E., Captain Webber, R.E., Messrs. Cromwell Varley, Willoughby Smith, and Latimer Clark. Mr. Sabine undertakes the two-fold office of Treasurer and Librarian, and Major Bolton is Secretary. The Society is composed, besides its council, of members, associates, and honorary members, to be elected from those only who have been regularly educated as telegraph engineers, or have acquired a degree of eminence in the profession. An annual provincial meeting is to be held in conjunction with the British Association. If talent will ensure a brilliant career, the association certainly promises well. There will be many who will watch its proceedings with great interest; a helping hand it can hardly want.

Mr. Joel has lately perfected a galvanometer for measuring the relative intensities of induced currents. The instrument is in principle somewhat analogous to the ballistic pendulum. On the axis of a pair of curved magnetised needles working between two vertical electro-magnets, is rigidly fixed a horizontal arm slightly curved upwards at its extremity. The extremity of the axis acts as a pivot to a slight ivory index loosely centred on an agate cup. By means of a micrometer screw, the index is brought against the curved extremity of the arm. If a current is now passed through the coils the index will be deflected, the arc varying as the intensity. The magnetised needles are arranged so as to register only the first current arriving in the coils. This form of galvanometer is simple, and seems calculated to meet with extended practical application. In connection with an induction coil, it would afford to the medical man a surer indication of the strength of the current he is employing than is at present available.

A novel adaptation of Sir W. Thomson's reflecting galvanometer has just been made. As is well known, the signals on a cable of high inductive capacity can be transmitted only with extreme slowness and precision, and even then the interpretation of a skilled clerk is necessary. It has been proposed, and tried experimentally with some success, to place at each end of the cable a system of clockwork to cause the synchronous movements of a fugitive dial, upon which are engraved the letters of the alphabet, appearing in succession, as the dial rotates, at an aperture in a fixed screen. On the same screen, a little lower than the aperture, the ray of light is reflected from the mirror of the galvanometer. The clerks at both stations, by pre-arranged signals, set their clockwork in motion at the same instant, the receiving clerk watching the dial and the light. The transmitting clerk, also watching his dial, immediately on the arrival of the letter required at the aperture, depresses a key, causing a deflection of the light at the distant station. Seeing this deflection, the receiving clerk immediately calls out the letter appearing at that instant on the dial to his assistant. According to experiments made on an artificial cable of extremely high inductive capacity, the rate of signals attained a maximum of 25 words per minute. Other advantages of the system are—that equal and opposite currents can be sent to line, and but a small battery power employed. A further modification is the substitution of type-wheels moving synchronously for the dials. Upon perceiving the deflection of the light the receiving clerk depresses a key, which causes a paper slip to be raised against the letter immediately opposite on the type-wheel. The difficulty of obtaining the perfect synchronism of the dials or type-wheels,

the inventor, or rather adapter, M. Poisset, says is apparent only, and has been combated in Professor Hughes's type-printer.

A modification of Steinheil's telegraph has been brought out by Mr. Herring. It is furnished with two keys, one to work a lever carrying a pin, to make a dot, and the other to work a lever carrying a small linear style, to print a dash. Greater accuracy seems likely to be secured, for it takes a long time to acquire the art of releasing or holding down the Morse key the proper interval, and generally learners find it easier to manipulate the single-needle than the Morse. It would also appear that increased speed is obtained, since the dash is printed in equal times with the dot. Mr. Herring suggests that it would be practicable to emboss two slips at the same operation, and to give one to the sender, who would thus know with certainty what messages had been dispatched. The instrument would be especially useful as a record in a single-needle or a bell circuit.

Mr. C. W. Siemens, F.R.S., in an interesting paper, read before the Royal Society, on "The Increase of Electrical Resistance in Conductors with Rise of Temperature, and its Application to the Measure of Ordinary and Furnace Temperatures; also a Simple Method of Measuring Electrical Resistances," showed that, according to the principle of the increase of resistance in a conducting wire with increase of temperature, an instrument could be constructed for measuring with extreme accuracy the temperature at distant or ordinarily inaccessible places, such as the top of a mountain, or the interior of a furnace where metallurgical or other operations are conducted. To measure temperatures not exceeding 100° centigrade, the apparatus is so arranged that two similar resistance coils are connected by a light cable containing three wires insulated from each other. These coils are termed "the thermometer coil" and "the comparison coil." The thermometer coil, carefully rendered impervious to moisture, is placed in the situation of which it is desired to ascertain the temperature; and the comparison coil is plunged into a test-bath, whose temperature is raised or lowered until an electrical balance is obtained between the two coils, as indicated by a galvanoscope. This equality of resistance can only take place when the coils are at the same temperature,—consequently the temperature of the test solution, measured by a delicate thermometer, is the temperature of the situation of the thermometer coil. The temperature of the connecting wires between the coils, which may be some miles apart, would materially affect the correctness of the measurements, if this source of error were not eliminated by means of a third wire connecting both coils of the instrument. The second portion of the paper is devoted to the description of an instrument for measuring electrical resistances without the aid of a magnetic needle or set of resistance coils, such an instrument being much called for on shipboard, where the motion renders delicate manipulation with nicely-balanced needles a matter of great difficulty. It consists of two graduated voltameter tubes, so connected that the battery current is divided between them, the branches including a known standard resistance and the unknown resistance to be measured. From this arrangement an expression is found for the unknown resistance, in terms of the standard resistance, by means of the volumes of gases evolved in n arbitrary units of time. Any change of atmospheric pressure affects both tubes of the voltameter equally; therefore any error from this cause is obviated. The upper ends of the tubes are closed by small weighted valves, which can be raised after each observation to allow of the escape of the gases without taking the instrument to pieces. Mr. Siemens has aptly termed this valuable invention "a differential voltameter."

Referring to Professor A. A. Mayer's method of fixing and exhibiting magnetic curves, Mr. C. J. Woodward writes to say that a somewhat similar method to the one Dr. Mayer describes is in use by Mr. W. F. Barrett, of the International College. Instead of using shellac, Mr. Barrett uses a solution of gum, which is allowed to dry on a piece of glass, and when the figures are

obtained they are fixed by breathing on the gummed glass. Mr. Woodward mentions, also, that he has used, at Mr. Barrett's suggestion, the lantern he devised for cohesion figures with very satisfactory results. This lantern allows the slide to be horizontal, so that the actual formation of the curves is seen on the screen when the slide is tapped. One of his pupils, Mr. Anderton, used, some years ago, a similar method to that of Dr. Mayer, only instead of shellac as a cementing material, canada balsam was used.

QUARTERLY LIST OF PUBLICATIONS RECEIVED FOR REVIEW.

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Report on the Progress of the Geological Survey of Ohio in 1869. *Columbus: Columbus Printing Co.*

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A Sketch of Romance of Motion, or a Mode of Motion of the Planetary Bodies in Space; also a Hypothetical Analysis and Synthesis of Nitrogen. By Alec. Lee. *Longmans and Co.*

An Illustrated Natural History of British Butterflies. By Edward Newman, F.L.S., F.Z.S. *William Tweedie.*

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Six Titles in Natural Law. By Richard Mansill.

The Subtropical Garden; or Beauty of Form in the Flower Garden. By W. Robinson, F.L.S. *John Murray.*

On Some Disorders of the Nervous System in Childhood. By Charles West, M.D. *Longmans & Co.*

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The Antiseptic System: A Treatise on Carbolic Acid and its Compounds. By A. Ernest Sansom, M.D. Lond. *Henry Gillman.*

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Öfversigt ap Kongl. Vetenskaps Akademiens Förhandlingar. *Stockholm: P. A. Norstedt.*

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The American Naturalist.

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The Geological Magazine.

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Memoirs of the Peabody Academy of Science. Vol. I. No. II.

Proceedings of the Bath Natural History and Antiquarian Field Club. Vol. II. No. II.

Proceedings of the Torquay Natural History Society.

The Thirty-Eighth Annual Report of the Royal Cornwall Polytechnic Society.

Proceedings of the Lyceum of Natural History of New York.

Transactions of the Woolhope Naturalists' Field Club for 1870.

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